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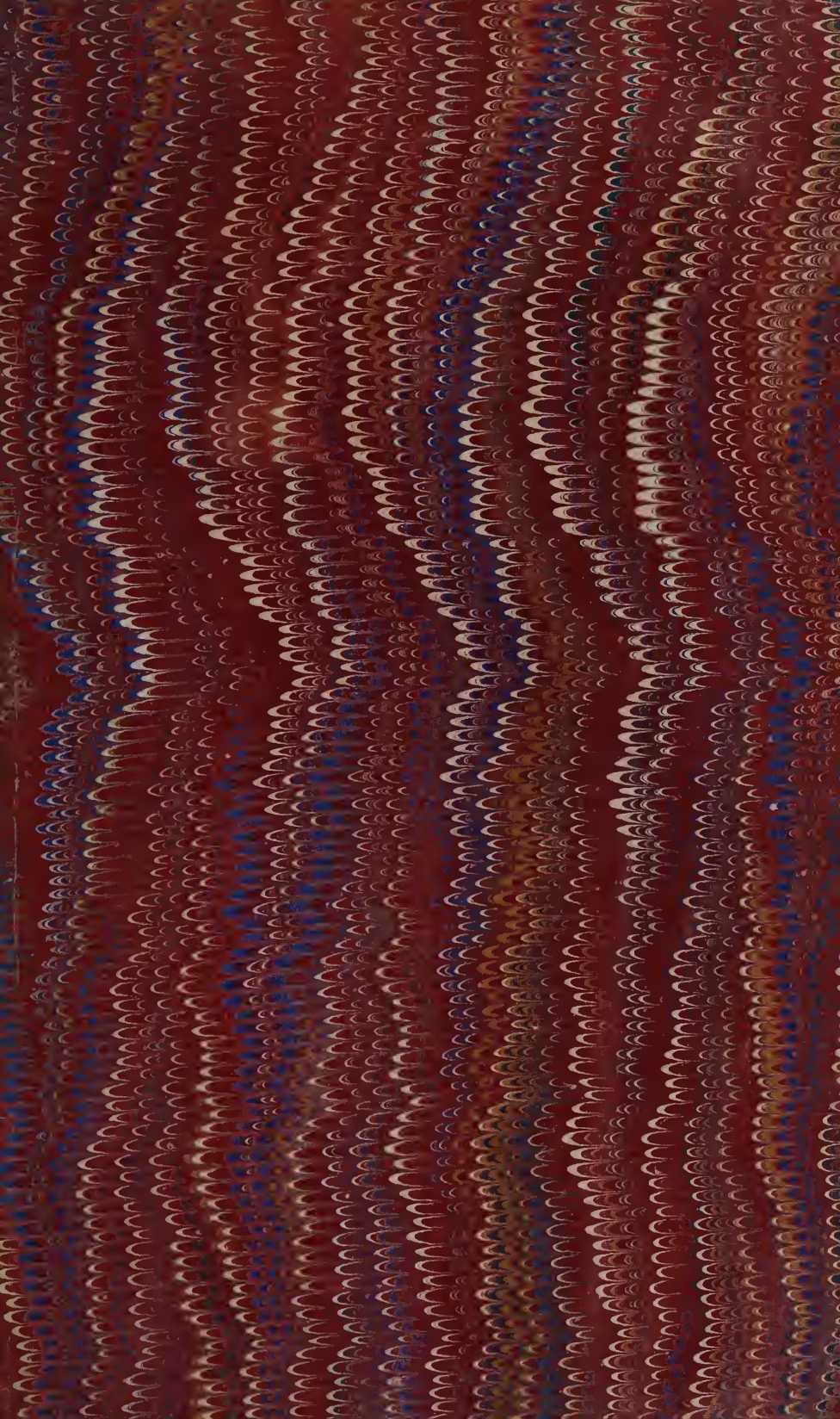
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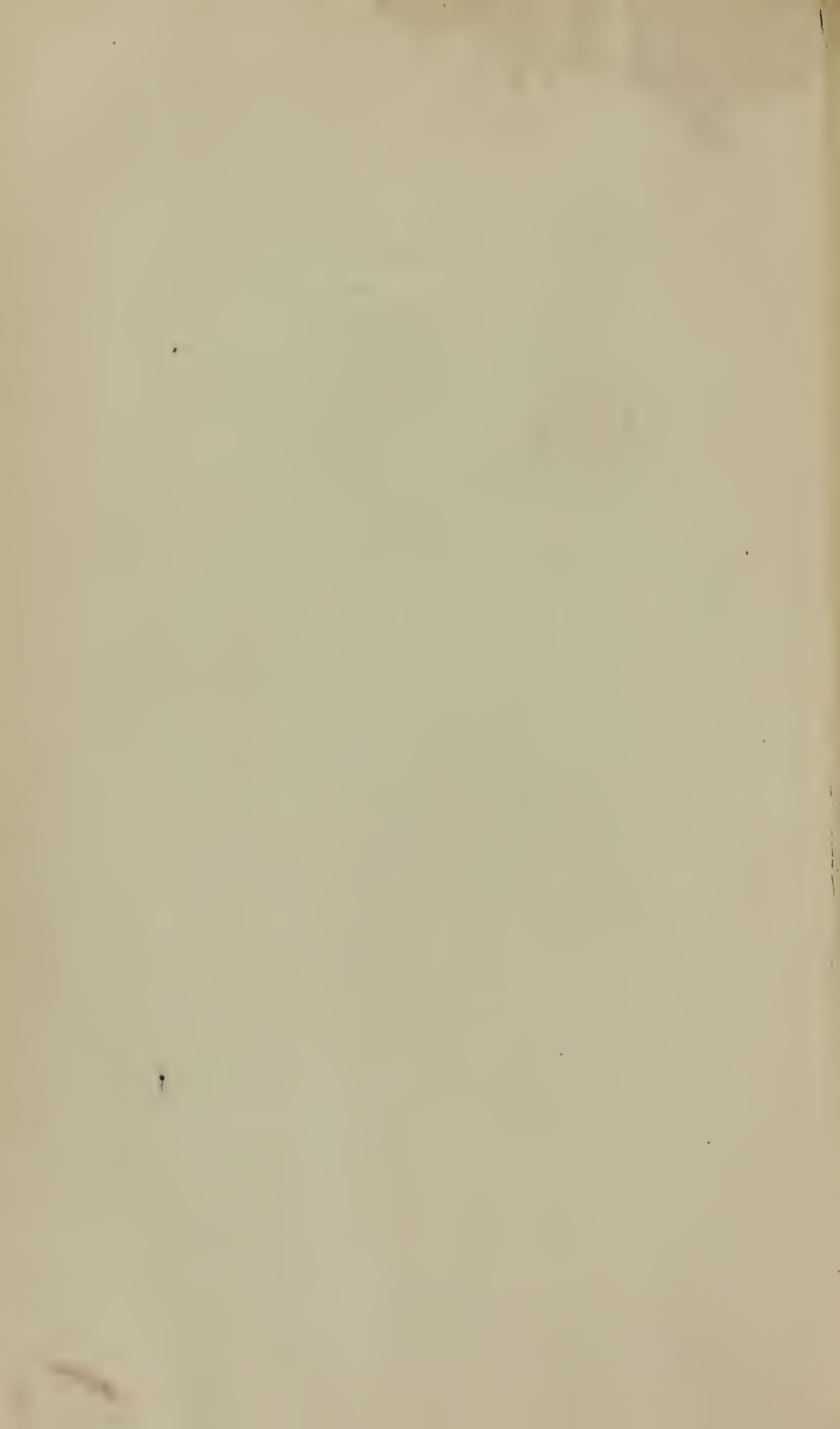
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DRUG ATTENUATION,

ITS

OBJECTS, MODES, MEANS, AND LIMITS

IN

HOMŒOPATHIC PHARMACY AND POSOLOGY.

BY

THE BUREAU OF MATERIA MEDICA, PHARMACY
AND PROVINGS, IN THE AMERICAN INSTITUTE
OF HOMŒOPATHY, 1879 AND 1880.

J. P. DAKE, M.A., M.D., CHAIRMAN.

Present by
the Author

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INTRODUCTION.

THIS volume, as the title indicates, is made up of the papers presented by members of the Bureau of Materia Medica, Pharmacy and Provings, at the meetings of the Institute held at Lake George and Milwaukee.

Taken together, they form a treatise on the subject of homœopathic pharmacy, especially upon drug attenuation, such as the profession has never had before, and such as cannot fail to be of interest to every student of the homœopathic method.

A great deal of mystery has hung about the object and methods of our drug management and dose preparation, owing to the use of such terms as "potentization," "dynamization," "spiritualization," etc. The object of the Bureau in entering the domain of Pharmacy, in 1879, was to prepare a clear and detailed account of the purpose of Hahnemann, and his methods, in attenuating drugs by trituration and succussion with neutral substances; and, also, to give the objects and methods of his followers, in the same work, since his time.

It was our desire to sweep away the mystery, as much as possible, from the entire business.

In 1880, our inquiry was, as to the extent to which the processes of attenuation should be carried. In solving this question we divided it in such manner as to bring out, first, the testimony of chemistry, of the microscope, and of the spectroscope, concerning drug presence in attenuations above the sixth decimal; then, the proofs of drug power, as furnished by physiological experimentation and analogy, in attenuations yet higher; and, finally, the proofs supplied by clinical experience, of medicinal

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efficacy in attenuations, from the sixth up to the highest made by the arts and appliances of pharmacy.

I regret to say that, owing to the failure of some members of the Bureau to do the portions of work assigned them, there has not been that full consideration of the difficulties attending clinical proofs and the care necessary to guard against unsound conclusions that our plan contemplated.

In conclusion I will say that, however much the fears of weak-minded men in the profession may have been excited by the announcement of our work, no charge of unfairness in our investigations or reports will be made by those who read and comprehend the contents of this volume.

Along with the papers of the Bureau I have brought the discussions thereupon which took place when they were presented to the Institute.

THE CHAIRMAN.

NASHVILLE, August, 1880.

REPORT

OF THE

BUREAU OF MATERIA MEDICA, PHARMACY, AND PROVINGS,

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T. S. HOYNE, M.D.,	Chicago, Ill.

TO THE

THIRTY-SECOND SESSION

OF THE

American Institute of Homoeopathy.

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INTRODUCTORY REPORT OF THE BUREAU.

By J. P. DAKE, M.D., CHAIRMAN, NASHVILLE, TENN.

DURING all the years in which this bureau has been charged with the duty of cultivating the fields of Materia Medica, Pharmacy and Provings, its attention has been given almost exclusively to Materia Medica and Provings.

As was once most aptly said, "the whole profession has seemed to make use of this bureau as a ready hopper into which to pour provings by the bushel."

The law *similia* accepted, the great aim has been to learn the positive or pathogenetic powers of numerous remedial agents. But in view of the uselessness of our therapeutic law and of our Materia Medica, where the means described in the one and pointed out as homœopathic by the other are so impure, or so undeveloped, or so imaginary, as to accomplish nothing of what is expected or desired of them, we have this year decided to devote the attention of the bureau entirely to a subject in pharmacy, namely, "*Drug Attenuation in Homœopathic Therapeutics.*"

In order to handle our subject systematically and thoroughly, we have divided it into five parts, as follows:

1. History of drug attenuation in homœopathic practice up to the death of Hahnemann, with a statement of its objects and methods.

2. History of drug attenuation in homœopathic practice since the time of Hahnemann, with a statement of its objects and methods, with especial reference to variations from those approved by Hahnemann.

3. The means employed in drug attenuation ; what they should be, and the dangers of impurity.

4. The limits of drug attenuation, or proofs of drug presence in attenuations above the 3d decimal, from the standpoint of the scientist.

5. The limits of drug attenuation, or proofs of the presence of medicinal power in attenuations above the 6th decimal, from the standpoint of the therapist.

Before proceeding with the papers upon these several parts it is proper for me to speak briefly of the leading purposes and methods in our treatment of the subject :

1. To properly understand the objects and applications of a measure or rule, in any department of science and art, it is necessary to learn its history, to consider the necessity and the circumstances leading to its adoption.

Recognizing Hahnemann as the father of drug attenuation, as he was of the *Materia Medica*, and of the therapeutic rule observed in homœopathic practice, we must examine his sayings and doings as illustrative of that measure in pharmacy.

Assuming his standpoint in the early morning of the homœopathic art, we witness the aggravations following his use of traditional doses in the sick. We see him diminishing his doses, dividing and subdividing the drug mass, in order to escape these aggravations. Step by step we follow him on to the point where he discovers the important fact that, the efficiency of the drug is not diminished in proportion to the diminution of its weight or bulk, its particles being very minutely divided, and so prepared for a more extended contact or a readier absorption.

We find him speaking of the development of latent or inoperative drug power by the process of trituration, or succussion, with neutral materials. And in his old age, prompted by enthusiastic admirers, we hear him speak of the mystic power, the "spirit of medicine," and of its marvellous increase by the gradual annihilation of drug matter.

The first paper to be presented will give you briefly the views of Hahnemann in reference to the attenuation and "potentization" of drugs.

2. Passing on, to a period not long anterior to the death of the

master and extending down to the present day, we find some of his disciples in the spiritual philosophy excelling him altogether. Spurning the material doses, with which he had demonstrated the reality of homœopathy, they have passed beyond all his calculations, into the shadowy realms of the unknowable, in the effort to grasp and wield the pure spirit of medicines.

Disdaining his sentiment when, planting himself upon the 30th attenuation, he said: "It must stop somewhere; it cannot go on to infinity," they have overleaped his painstaking methods, and gone into all manner of "bottle-washings" and shorthand ways of "potentizing" drugs, not to the thirtieth degree only, but, as claimed, to the millionth!

The second paper presented will bring to notice the departures from the pharmæcal teachings of Hahnemann, with the reasons therefor, so far as made known.

3. The right action and efficiency of doses employed in homœopathic practice require the utmost care in the selection of neutral materials and of mechanical appliances and measures in pharmacy.

If we would have a given quantity of the remedy, prescribed in each dose, have it in a certain state of comminution, and pure, it is necessary to observe every possible precaution in regard to methods and materials, employed by Hahnemann, or suggested by discoveries and improvements made since his time.

The dangers of impurity and of defective methods and means will be pointed out in the third paper presented.

4. From the time when Hahnemann began to attenuate drug matter, in the reduction of doses and development of latent drug power, down through all the history of homœopathy, in every country where it has appeared, the most determined and effective opposition to its acceptance has been directed against the small doses employed.

It is now an old and quite traditional objection to our system, that we believe in the philosophical absurdity, "The less the cause, the greater the effect." And the charge, equally old, has been everywhere repeated, that our doses are entirely destitute of medicinal energy, and totally inert. To meet such objections

our methods of attenuating drugs, and the objects in view therein, have been set forth and defended time and again.

Efforts have been made by many of our writers to show the reasonableness as well as the utility of our pharmaceutical measures and of our small doses, and yet, comparatively, but a small portion of the medical profession and of the laity have been convinced of the soundness of our position and the superiority of our practical measures, even in countries where the progress of homœopathy has been the most rapid.

In view of the diversity of opinions held, and of proofs adduced, by authors and teachers in our school, regarding the attenuation of medicines, and especially in view of the extreme positions assumed by some who would "go on to infinity" with the work of attenuation, calling it "dynamization," it is proper for us, here, to inquire after that class of proofs the stability and value of which all may appreciate and none question.

We come to ask the scientist, what his means and methods reveal in regard to the attenuability of drug matter, or how far up our decimal scale he can trace the presence of our medicines. Chemical tests go beyond the sense of taste and of smell and of ordinary vision. The microscope and the spectroscope go yet farther, telling us of drug presence in the 6th, 8th, 9th, and some claim in the 12th, attenuation. Physiological tests and provings go farther still, while analogy tells of the ability of various imponderable miasms and unseen germs to influence the bodily conditions of man in ways very decided and to extremes very fatal. The evidences thus furnished are well understood by learned men and will, by them, be forced upon an incredulous medical profession.

The papers relating to the fourth division of our subject will furnish an exhibit of such data, from the standpoint of the scientist.

5. Passing on, we have also to inquire what proofs are furnished in the medical field, viewed from the standpoint of the *therapeutist*, that there is medicinal power in attenuations, in which the scientist can discern no medicinal matter

We must remember how much the sensitiveness of the human organism to the homœopathic remedy is increased by disease,

and, consequently, how much more delicate the test afforded by clinical experience than any furnished by extraneous means.

But, in applying this more delicate clinical test, we must likewise remember the complications and difficulties which unavoidably attend it. It is not as though one chemical element were introduced to detect the presence of another, nor as if a simple object were brought under the microscope for us to determine its visibility, nor even, as if a drug were introduced into a healthy organism, that we might learn its pathogenetic character.

In the administration of a medicine to the sick we are placing an agent amid the complex and delicate machinery of an organism, already disturbed by other agencies, termed morbid causes, and continually subject to the many minor disturbing influences of personal environment.

In looking upon the symptoms, appearing in the drug prover, it is found exceedingly difficult, in fact impossible by one experiment, to determine which are really drug-effects and which the effects of inherent or circumstantial causes. Such being the case, where no special morbid factors are operating, how much greater the difficulty when we wish to determine how much, or if any, definite impression is made upon a diseased organism by a medicine administered. It will not do to assume that the disappearance of disease, or recovery of health, in every case subjected to our treatment, is due to the doses of medicine administered. Logic and common-sense, alike, cry out against such arrant presumption.

In view, then, of the uncertainties attending the testimony of clinical experience, we can but look with great doubt and disfavor upon the practice, too common with authors in our school, of gathering out of the journals and society transactions every reported cure, as the certain effect of a medicine administered. The idea of extending our *Materia Medica* by the addition of all the new symptoms appearing in the sick, as if they were the positive effects of the medicine given, is too absurd for a moment's serious consideration. And the case is not a whit better when all the symptoms disappearing from the sick are added, now as positive, or again, as curative symptoms of the drug employed.

Planting ourselves upon this solid ground we cannot hesitate to declare Allen right in excluding, from his *Encyclopedia*, all symptoms reported, as appearing for the first and only time in the sick, after the administration of certain drugs; and Hering wrong in hoarding up, as reliable verifications, and as "guiding symptoms," all those symptoms by anybody reported, as disappearing from the sick, after the exhibition of homœopathic remedies.

When, now, we attempt to gather up proofs from the therapeutic field as to the presence of medicinal power in attenuations, which the scientist says show to him no medicinal matter, we must proceed with extreme caution. It will not do to take the reports of medical enthusiasts, who claim and publish a cure, an undoubted medicinal effect, in every case where the highest attenuation has been administered and the patient has recovered. Who has not learned that the *post hoc* is not always, nor generally, the *propter hoc* in medicine?

Reports may be taken, as of some worth, from practitioners thoroughly educated in the elements of medicine, acquainted with the natural history of diseases, so as to know what symptoms belong to them, and so familiar with the general range of medicines employed as to distinguish their effects from the ephemeral effects of surrounding influences.

Reports may be valuable, coming from wise and cautious and conscientious practitioners, realizing the difficulties of their undertakings, the chances for error, and the supreme importance of the truth, and nothing but the truth, in journals and books calculated to govern human effort in behalf of human life.

It is for us to scrutinize reports for internal evidences of value, and we may not be excused from carefully considering the character and qualifications of their authors.

It is well known that every theory in therapeutics, as well as every noted remedy and measure mentioned in history, has had abundant testimonials in its favor. Clinical experience has testified in behalf of the most arrant humbugs and impostors the world has ever seen.

Hence, when we pass beyond the checks and safeguards of scientific observation and analogy, when we come to prescribe

doses for the sick, in which the scientist and even the mathematician says there is no medicine, we are compelled to be incredulous enough to look closely within and about our patient, to determine if, possibly, some other influence, beside that of the drug, may not be playing the chief part in the removal of disease or restoration of health.

The papers, relating to the fifth division of our subject, will submit what proofs we have of drug power in attenuations above the 6th decimal, as furnished from the field of therapeutics.

In conclusion, as to the grand purpose of the inquiries instituted by our bureau, I should say, that it has been to obtain all possible proofs of power and efficiency in our attenuated doses. While interrogating the scientist we have not failed diligently to inquire of the practitioner of medicine; nor have we failed to consider the invisible and imponderable forces of nature while seeking proofs from the experimental physiologist. If, in our work, any bias or leaning to one side or the other has led us astray it has been to the side of attenuated doses.

DRUG ATTENUATION IN HOMŒOPATHIC THERAPEUTICS.

FIRST DIVISION.—HISTORY OF DRUG ATTENUATION IN
HOMŒOPATHIC PRACTICE UP TO THE DEATH
OF HAHNEMANN, WITH A STATEMENT
OF ITS OBJECTS AND METHODS.

BY J. P. DAKE, M D.

FROM the year 1796, when homœopathy was born, to 1801, a period of five years, Hahnemann made use of the ordinary preparations of the apothecaries in the treatment of the sick. In no wise did he depart from the usual style of doses, except in having but a single drug in each and in their gradual diminution, under the teachings of experience.

In 1801, in his essay upon the "Cure and Prevention of Scarlet Fever," speaking of Belladonna, he said, "The very smallest dose should be administered." And in the same connection, answering the charge of inefficiency of the doses employed, he adds: "Moreover it is probable that the thorough admixture of the few drops with a sufficient quantity of the fluid, in which it should be taken, was generally neglected."

Here is the beginning of drug attenuation as followed in the homœopathic school.

The first idea, evidently, was a diminution of dose to avoid medicinal aggravation, and the second was, the development of drug power by the repeated division of drug particles.

The division of particles had two objects in view; first, to prepare the medicine for a more ready absorption and conveyance to the seat of disease; and secondly, to increase its surface for a more extended and effectual contact with nerve-tissue.

The first formula, for the attenuation of drugs, mentioned by Hahnemann was to "mix intimately one drop of the tincture with five hundred drops of diluted alcohol; and then one drop of this mixture with five hundred other drops of diluted alcohol; shaking the whole well."

In this way he had Opium prepared for use in scarlet fever. Ipecac. he prepared by mixing one drop with one hundred drops of dilute alcohol, and Belladonna by using four hundred drops of the dilute alcohol for the first attenuation, three hundred for the second, and two hundred for the third. Chamomilla he prepared by putting one drop of the tincture with five hundred drops of water and five hundred of alcohol, for the first attenuation, and eight hundred drops of dilute alcohol for the second.

Such formulæ surprised the apothecaries, and led the famous Hufeland to ask, "What effect can the hundred-thousandth part of a grain of Belladonna have?" Hufeland's *Journal*, in which this question was propounded, contained the answer in clear and vigorous terms.

The difference between the medicinal activity of "a very hard, dry pill of Belladonna," when swallowed whole, and the same amount of that drug taken in "solution," and likewise the difference between the sensitiveness of the human organism to that agent when in a state of robust health, and when diseased, so as to call for it as the homœopathic remedy, Hahnemann pointed out most clearly.

In his reply to Hufeland he shadowed forth a theory, in regard to drug action, which afterwards became his famous doctrine of *dynamization*. He said: "Medicine does not act atomically, but only dynamically;" or, as afterwards explained, not according to its matter, but its spirit.

In his famous essay, "The Medicine of Experience," published in 1805, and afterward enlarged into the first edition of the *Organon*, he said: "This *dynamic* action of medicines, like vitality itself, by means of which it is reflected upon the organism, is almost purely spiritual in its nature."

But it must be noticed that, in all his references to attenuated doses, he speaks of them as still possessed of some fraction of the original drug matter.

Down to the year 1825 there seems to have been no essential change in his objects or methods in the attenuation of drugs. He still wished to develop and control drug power.

When seventy years of age, in answer to the inquiry, "How can such small doses of such very attenuated medicine as homœopathy employs still possess great power?" he brought his *dynamization* theory into full view. From that time on we often find him writing of the *spiritualizing* effect of repeated and thorough triturations and succussions.

Adhering to the formula, one drop or one grain of the medicine to one hundred or to ninety-nine drops or grains of the menstruum, he went up, step by step, to the 30th attenuation, or, as he came to say, the 30th "potency."

Again, I must observe, however, that Hahnemann, down to the day of his death, held to the belief that, in every attenuated dose, some drug-matter was yet present. In the *Organon* this statement from his pen was ever standing: "A substance, divided into ever so many parts, must contain in its smallest conceivable parts still some of this substance, and the smallest conceivable part does not cease to be *some* of this substance."

I leave to other members of the bureau, who report on this and on the second division of our subject, to mention variations, not to say contradictions, in the teachings of Hahnemann, and to show how other persons, taking up the dynamization theory, carried it to the extreme of absurdity, throwing around homœopathy those vast clouds of mysticism which have overshadowed its grand truths and cast suspicion upon its great works, as viewed by the greater part of the medical world.

In closing this brief paper I must explain that I have dwelt much upon the views and practices of Hahnemann not because I consider them in anywise authoritative or binding upon us, but simply as a matter of history, and a means of illustrating what is meant by drug attenuation and potentization.

I do not hesitate to declare that I have much greater confidence in the convictions avowed by Hahnemann when in the full vigor of ripened manhood, than when, in the gathering frailties of old age, the bitter opposition of enemies and the flattery of friends alike took him to extremes.

HISTORY OF DRUG ATTENUATIONS AFTER THE TIME OF HAHNEMANN.

BY W. L. BREYFOGLE, M.D., LOUISVILLE, KY.

HAVING been assigned the second part of the special subject of this bureau, viz.: "The History of Drug Attenuation in Homœopathic Practice since the Time of Hahnemann, with a Statement of its Objects and Methods, with Especial Reference to Variations from those Approved by Hahnemann," I find myself confronted by many difficulties, and if, in my feeble attempts to describe the different methods of attenuating or potentiizing medicines, I fail to give credit to the proper sources, or should seem to do any one injustice, I beg you will take into consideration the following facts:

First. That there are no publications which give the history of the different methods stated; on the contrary, some few of the many who have placed their preparations before the profession, have kept secret the particular methods followed, thus compelling the profession to trust them on the merits of their labels alone.

Second. That no distinction seems to have been made between the numbers, as used by Hahnemann in defining his attenuations, and as employed for those made by methods essentially different from his.

According to Hahnemann one grain of the medicine thoroughly triturated with one hundred grains of the vehicle was labelled the "hundredth;" one grain of this triturated with one hundred grains of the vehicle was labelled the "ten thousandth;" one

grain of this preparation, triturated with one hundred more of the vehicle was labelled the "millionth." This latter preparation was then the "3d centesimal trituration." The number "one millionth" being used to determine the fractional quantity of medicine it contained.

From this 3d trituration all of Hahnemann's dilutions were made, and he frequently referred to it as the "mother of dilutions." When a dilution was required, one grain of this 3d trituration, or mother, was mixed first with fifty drops of water; and when dissolved fifty drops of alcohol were added, and it was labelled the "one one-hundredth," one drop of this added to one hundred drops of alcohol, was labelled the "one ten-thousandth," and so on.

When, therefore, we contrast this method of numbering with that afterwards adopted, viz., of indicating each separate dilution by figures, as one, two, three, etc., up to the millionth, the latter preparation representing not division of particles, but a million consecutive dilutions, you will readily understand how difficult it is to distinguish between and to describe the different methods of attenuation. And you will perhaps discover also, that, what Hahnemann chose to dignify by the title of "high potencies," differed greatly from those of the modern "homœopathician."

For many years, by the advice of Hahnemann, attenuations were not extended above the thirtieth; and in order to establish some uniformity in preparation, these thirtieths were all made as described above.

Experiments were then made, which will be described in another section of this bureau, by Korsakoff, Jenichen, and others, who were the *real* inventors of "high potencies."

Jenichen kept secret his method of preparing them, but from bits of information gathered from his letters, he depended upon continuous succussion without dilution, and reckoned the number of "potencies" by the number of shakes he gave the bottle, ten shakes being equivalent to one degree of "potency." Jenichen was a horse trainer by profession, and shot himself, we learn, after making the sixty-thousandth potency!

The decimal scale, I am informed by Dr. F. E. Boericke, was

first mentioned in an article by Dr. Hering, printed in *Archiv XIV*, 2, page 134, where he alludes to experiments made, which showed that remedies acted differently when attenuated one to ten, one to one hundred, and one to one thousand. That the action of those attenuated with a larger quantity of vehicle seemed to be more superficial. On the appearance of this article medicines were prepared by many according to the decimal scale, and the practice spread so rapidly that Gruner, in his *Pharmacopœia*, adopted the decimal scale as a basis.

In the history of drug attenuation, since the time of Hahnemann, we have records of those made by Fineke, Dunham, Lehrman, Boericke and Tafel, and Swan.

Fineke has never published anything descriptive of his process of making "high potencies," preferring to keep all such information to himself. Sufficient is known, however, from the Patent Office to justify the assertion, that they are made by the fluxion process, and with water.

Dunham and Lehrman both prepared their high attenuations according to Hahnemann's plan, and with water. The same is true of Boericke and Tafel's preparations, except that they use alcohol.

Swan prepares his also with water and by the fluxion process. A machine is used ("Dr. Skinner's Centesimal Fluxion Potentizer," a description of which is given in the new medical journal called the *Organon*, No. 1, vol. 1, page 45), capable of filling and emptying a vial fifty times per minute, seventy-two thousand times per day, a million times in fourteen days and a half, running night and day. The glass is impregnated with one drop of the 1st centesimal dilution, then emptied and placed in position, the hand on the dial plate placed at O, and for every time the vial is emptied and filled the dial registers one "new potency." The operator is directed to turn the driving-wheel (ordinarily run by water) by hand until the first ten "potencies" are made, "as it is much safer"—(just why it is safer the doctor does not state). After this is accomplished, full power is turned on, and the vial is filled and emptied fifty times per minute, making a dilution one degree higher at every revolution. At the end of two weeks the hand on the dial reaches the number

one million; then the hand must be brought back to the beginning, and in due time it produces the two, three, or four millionth, as required. I notice one case reported cured by the fifteen millionth!

This machine certainly deserves great credit as a labor-saver, for, according to the plans adopted by Hahnemann, Dunham, Lehrman, and Boericke & Tafel, it would require the constant labor, night and day, Sundays included, of one man for twenty-eight years to carry a single remedy up to the fifteen millionth dilution. It would cost over \$100,000 for alcohol, and \$33,000 for labor, at 15 cents per hour.

The object sought to be obtained by the attenuation of drugs was, according to Hahnemann, "to prevent serious aggravations from too strong a dose of medicine, prescribed strictly in accordance with the homœopathic law" (see § 275, *Organon*).

In § 279 he says, "The dose of a homœopathically selected remedy cannot be reduced so far, as to be inferior in strength to the natural disease, and to lose its power of extinguishing and curing at least a portion of the same, provided that this dose, immediately after having been taken, is capable of causing a slight intensification of the symptoms of the similar natural disease." In the preceding paragraph, however, he admits that "to determine the dose of each particular medicine for this purpose, and how to render this dose so small as to accomplish its purpose, gently and rapidly at the same time, is a problem which obviously, is neither to be solved by theoretical conjecture, nor by sophistic reasoning."

It would seem, from these extracts, that the object of attenuating was to prevent too great medicinal aggravations, but, that to know precisely which attenuation was to accomplish this, in each given case, was a problem "obviously beyond all solution." This question Hahnemann himself never solved; his favorite potency, in his old age, being the 30th, although he often went lower and sometimes higher, as his cases seemed to demand. Having some curiosity to learn whether Hahnemann changed his views on this subject, during the latter part of his life, I addressed a few questions to Madame Hahnemann, and from her letter, received a short time before her death, and which now lies

before me, I quote the following: "You ask if Dr. Hahnemann changed his opinions in the last phases of his life on the potencies? and if he always administered high dynamizations?" "He used all the dynamizations, high and low, as the case of his patients required. I have even seen him give the 3d trituration, 'mother,' where he found it necessary, as he also administered the 200 milliems or thousandths when it was necessary."

It is urged by some, that by triturating and suceussing remedies their strength is increased. Repeated efforts to dilute medicines have caused some enthusiastic supporters to attribute to their effects a real spiritualization of the drug property—a true, astonishing, unveiling, and vivifying of the medicinal spirit. A medicinal power, it is claimed, is liberated from its material bonds, so as to enable it to operate more penetratingly and more freely; and yet, the material receptacle of these spiritual forces, the palpable ponderable tissues are not to be taken into consideration!

This theory, however, involves a contradiction. Hahnemann says, for instance, "that the weakest possible dose is sufficient to overcome the disease," and that the dose can scarcely be made weak enough to avoid aggravations. On the other hand we are told that the process of attenuating drugs, instead of diminishing, really increases their medicinal action. Exactly which "horn of this dilemma" we will take has not yet been concluded.

As we have seen, in the manner of preparing attenuations, a wide difference of opinion exists; and it seems difficult to state precisely what constitutes a potency. Hahnemann tells us that one grain of the 3d centesimal trituration in ninety-nine drops of the vehicle is the "one one-hundredth," one drop of this in ninety-nine of alcohol is the "one ten-thousandth," and so on. Jenichen tells us that the 1st centesimal dilution becomes a new "potency" after every ten vigorous shakes, that three hundred shakes make the 30th "potency," and six hundred thousand shakes make the "60,000th potency." Drs. Dunham and Lehrman claim that each attenuation should be made separately, and prefer water as a menstruum.

Boerieke & Tafel also make each separately, but prefer alcohol as the vehicle.

Dr. Fincke, sharper than the rest, keeps his process to himself, expecting us to keep our eyes on the label, and to trust in God for the results.

Dr. Swan places a vial which had previously contained a drop of medicine, but which is now emptied, into position, turns loose a hydrant upon it, and at the end of fourteen and one half days he presents us the "1,000,000th potency;" or, if allowed to run steadily night and day, Sundays included, in precisely seven months and fourteen and one-half days he produces that monument to his genius, the "15,000,000th potency," through the use of which the *Medical Investigator*, in a recent number, reports a brilliant cure.

The question of potency has not been, nor does it seem likely to be settled by current methods. Even Hahnemann's views underwent frequent changes in regard to attenuating drugs. He first advised that each dilution be shaken for a moment or two only; afterwards warns us against too much shaking, as calculated to increase the power to a dangerous extent, and that only two shakes should be given each dilution. He afterwards loses his dread of shaking and appoints ten shakes for each dilution, as a standard; and again he allowed twenty, fifty, or more shakes, and half a dozen before each dose. In one place he says that the shaking is the only agent in the dynamization, and that a strong medicinal solution, if shaken long enough, will become like the 30th potency; in another he alleges that dilution is essential to the dynamizing of succussion, and that all the rubbing or shaking in the world will not dynamize an undiluted substance.

Notwithstanding this declaration we find Jenichen giving as many as six hundred thousand shakes to a single bottleful which he sent abroad as the "60,000th potency," and which, with many others made by him, now form part of our armament. Jenichen tells us he worked five hours a day at his preparations. Supposing each stroke occupied a second of time, and he worked continuously during all these five hours without stopping for an instant, to carry up one single remedy to the sixty thousandth

would take him nearly five weeks of hard labor. We are not astonished when we hear him denounced as a "disreputable catchpenny, puffed into unmerited notoriety by a few credulous homœopathists, who should have known better than to lend their reputations to the propagation of what five minutes' calm calculation might have convinced them was an impossibility and a cheat."

There also exists a decided difference of opinion, due to the misapprehension of the word "Hahnemannism," which name, furnished by Dr. Hering for the new and unheard of natural force developed in attenuating drugs (just as the terms Mesmerism and Galvanism were used after Mesmer and Galvani), was intended to apply to any and all attenuations, the third as well the thirtieth. As the science of homœopathy progressed, however, it was made to do another duty, and is now applied only to the higher attenuations.

The word "Hahnemannian," also, was first used to designate all those physicians who adhered closely to Hahnemann's teachings. No matter what the attenuation prescribed, so long as they practiced according to the law of the similars, they were entitled to protection under that banner. Modern usage, however, has made it a notorious prostitute. It now represents a refuge for the vendors of secret nostrums,—for the theorist, who openly proclaims the fact that he does not follow Hahnemann's instructions,—as a cover for the surreptitious introduction into homœopathy of all sorts of "isms;" as an "ark of safety" for the ignorant pathologist, who is ashamed to be detected with any other book in his hand than a *Symptomen Codex*!

It is the barrier behind which the puny adversary gathers his little mud balls, ready to soil and blacken the character, perhaps, of him whose devotion to the law of similars has prepared the very soil upon which he has pitched his tent. Gentlemen, Hahnemann himself was not a "Hahnemannian" in this modern acceptance of the term.

Another evil which the introduction of "high potencies" has developed has been the speculative tendency. The development of this evil, like the rapid rising of a mighty river, will, unless relief soon appears, surely overwhelm and destroy us.

Think of it! A stream of hydrant water is allowed to run into an empty vial for seven and one-half months, night and day, Sundays included, without interruption, and we are presented (for a sufficient consideration) with the "15,000,000th potency," with which we are assured Dr. Bolus, of Bungtown, cured a remarkable case that had resisted the treatment of six or seven hundred allopaths!!

But the climax of unblushing effrontery is reached when the announcement is made "that as the pathogenesis of these extremely high potencies differs greatly from those of the crude drug, new provings should be made!"

It is time to stop and consider. We must, if possible, retrace some of our steps, and we must learn to call things by their right names. We must not in future be dazzled by the brilliant rays of "Skinnerism," nor by the seductive flattery of "Swanopathy." We must rally again around the old flag of homœopathy, and although she is somewhat soiled and battle worn, we will bear her aloft in our efforts for new victories and fresh laurels.

THE MEANS EMPLOYED IN DRUG ATTENUATION;

WHAT THEY SHOULD BE, AND THE DANGERS OF IMPURITY.

BY LEWIS SHERMAN, M.D., MILWAUKEE, WIS.

HAHNEMANN'S method of drug attenuation has, with commendable strictness and honesty, been adopted by the pharmaceutists of the homœopathic school.

Modifications of this method have in no wise affected the original design. They are: 1st. The use of the *decimal scale* in the place of the centesimal in the preparation of the lower attenuations. This insures a more through subdivision of the particles in the process of trituration, and accommodates those who prefer to administer the lower attenuations, by making preparations intermediate in strength between the crude and the $\frac{1}{100}$ th, one between the $\frac{1}{100}$ th and the $\frac{1}{10000}$ th degree of attenuation.

2d. The use of *precipitated metals* in place of the coarser preparations used by Hahnemann. This insures greater purity and more complete subdivision.

3d. The use of *yellow glasses* to contain and preserve the medicines. It is a fact well known to scientists and easily proven to one not versed in optics, that the rays of light tend to decompose and otherwise chemically change nearly all medicinal substances. It has been found by experiment that this power to effect chemical changes resides almost wholly in the short rays, viz., the green, blue, indigo, and violet, the red, orange, and yellow having scarcely any of this power. It has been found, further, that the

illuminating power of light is strongest in the yellow ray. Hence the advantage of glass which transmits only yellow light; for it gives the *maximum* of illumination offered by any single ray and almost a *minimum* of chemical action.

This fact may be illustrated by a simple experiment, which any physician can perform in his office.

Take vials of white, yellow, blue, and blackened glass; put into each the same number of pure sugar globules; moisten the globules in each with an equal quantity of an alcoholic solution of Nitrate of silver of the strength of $\frac{1}{100}$ th; set all the vials in the window and observe the change in the color of the globules. The proportion of chemical action will be found to range nearly as follows: White 100, blue 95, yellow 5, blackened 5.

4th. The selection of the *appropriate solvent* in the preparation of tinctures and dilutions. The importance of this precaution has been almost ignored by pharmacutists and physicians. The British Homœopathic Pharmacopœia, 1876, however, recognizes the fact that drugs of different nature require different solvents to extract their medicinal properties, and gives some specific directions. It is certainly absurd to prepare an aqueous solution of a resinous plant or an alcoholic solution of a substance insoluble in alcohol, or to make the lower dilutions without reference to solubilities. If we want reliable solutions of drugs we must find good solvents.

Other modifications of Hahnemann's formulæ have been made by persons having more or less influence, which have not received the sanction of the main body of the intelligent disciples.

The most important is that attributed to Count Korsakoff. It may aptly be termed—

THE BOTTLE-WASHING METHOD.

All of the dilutions of one remedy are made in *one* bottle by putting in 1 drop of tincture, and adding about 100 drops of alcohol and shaking, then turning the bottle upside down and throwing away all which does not adhere to the inside; then adding another 100 drops of alcohol, shaking and pouring out, etc. This might seem an innocent method of saving time and

expense and getting *almost* as good a preparation as could be made by Hahnemann's process. Compared with the master's plan it is slovenly and inaccurate. As a matter of fact it was the forerunner of a series of departures which have done much to disgrace homœopathy.

The substitution of *wedgewood* for *porcelain* mortars and pestles is not in accordance with the letter or spirit of Hahnemann's method. Let any one take a new wedgewood mortar and briskly rub the inside with the pestle for a few minutes. The cloud of dust and stench which arises to his nostrils cannot fail to convince him that the 3d centesimal trituration of *Calcareo carbonica*, made in such a mortar, will be as likely to represent the medicinal properties of *Silicea*, or *Hepar sulphuris calcareum*, as those of the remedy he desires to attenuate.

We come now to consider some of the departures from the strict rules of homœopathic pharmacy in the matter of making attenuations.

CONTAGION POTENCIES.

The first great departure was made by an amateur in medicine, a Sarmatian count,—Korsakoff by name. "He placed in a bottle 1000 sugar globules, unmedicated, and added to them 1 globule mediated with the 100th of Sulphur, and shook the whole for a minute, when, of course, he found that any of these globules, thus infected by the mediated one, acted just as well as if it had been medicated with the tincture at first hand. His next feat was to mediate in the same manner 13,500 plain sugar globules, by shaking them for five minutes with 1 globule of Sulphur 30th.

Dr. Gross took up the idea and soon notified the medical profession that he had communicated blood-power to ever so many globules, by adding to them 1 globule moistened with a dilution of his own blood. With this spiritualized blood-power, he cured congestion. Dr. Plaubel, of Gotha, said that it did not matter if the globules fell out of the powder of milk sugar in which they were administered, for that sugar was already infected with the medicinal virtues of the globules with which it had been in contact.

SUCCUSSION POTENCIES.

This, the second great departure, was also introduced by a layman in medicine, a very muscular horse-trainer, employed in the stable of the Duke of Gotha.

This innovation was purely a financial scheme. The inventor therefore kept his method a secret as far as possible, and working on the credulity of such men as Drs. Gross and Hering, gained a wide reputation, and accumulated a fortune by the sale of his potencies. From so much of his process as has leaked out it appears that the essential feature of his plan was to abridge and cheapen Hahnemann's process by using only one bottle for all the potencies and omitting to empty it, depending on the succussion to effect the potentizing. Every *ten* shakes was counted a potency. In pursuance of the request of Dr. Hering he went up in this way from 30 to 100, to 200, 500, 800, 1000, 1500, 2000, 10,000, 50,000, and finally to 60,000. This last potency was the result of 600,000 shakes of his own "powerful arm."

FLUXION POTENCIES.

The next departure of note was made by Dr. B. Fincke, a high potency homœopathic physician of Brooklyn, New York. This also was intended as a money-making scheme. The inventor patented his process, thinking this safer than to attempt to keep it a secret. Like the succussion plan of Jenichen, the fluxion idea was the daughter of laziness and greed, and found its ally in ignorance and superstition. This process dispenses with the *bottles*, the *alcohol*, and the *shaking*, and employs instead a small tumbler and a quantity of ordinary hydrant water. A measured quantity of water, containing, besides many impurities, appreciable quantities of *Calcarea carbonica*, *Calcarea sulphurica*, *Natrum muriaticum*, and other familiar medicinal substances is allowed to flow from a tank from a height into a tumbler containing a hundred drops of the 1st dilution of the medicinal substance. The tumbler is filled and allowed to overflow, while the measured quantity of water runs in. As the process continues the medicine is supposed to get at the same time scarcer

and stronger. For every drachm of water which runs in there is tallied one degree of rise in potency. If a thousand drachms have run in the liquid is called the "1000th potency." If ten thousand drachms, the 10,000th potency. A little calculation will easily show that these attenuations are not centesimal, and that they do not correspond in any way with Hahnemann's preparations. Further, it is evident that no two potencies of the same number will be *alike*, for the proportion of medicine and water in the overflowing liquid varies with the force of the stream and other conditions which it is impossible to estimate. Consequently they do not represent any definite and determinable medicinal strength. The *thirtieth* potency of Fincke, made in a one and a half drachm tumbler, should, theoretically, be equivalent in drug strength to the *sixth* of Hahnemann, but, practically, it would vary between the *second* and the *tenth*. Pharmaceutically speaking, these preparations can only be characterized as *cheap* and *nasty*.

According to Dr. Samuel Swan, a notorious advocate of the fluxion potencies, if a drop of the 1st dilution be treated with 1,000,000 drops of water, it will be raised in potency 1000 times on the centesimal scale. Perhaps he meant 100,000 drops of water. In either case the statement is very far from the truth. The same Swan says that $333\frac{1}{3}$ cubic inches of water is all he uses to make the 1000th potency, and that $333\frac{1}{3}$ cubic inches of water contain 1,000,000 minims. A simple calculation shows that this quantity would contain only 88,658 minims; hence we infer this man to be a base pretender, for if he could not make the computation himself he ought to have hired a boy to do it for ten cents.

Dr. Boericke, of Philadelphia, has constructed a potentizer in which fluxion and succussion are combined. The impure Schuylkill water, which does the diluting, runs through a rubber tube into the potentizing glass.

Dr. Skinner, of Liverpool, England, has invented a potentizer in which the water passes through brass tubes into the potentizing glass. This glass is emptied at each rise in potency. But the most interesting feature of this system is that the *same glass* is used for the preparation of the potencies of all the different

remedies in succession. Dr. Skinner states that he first puts into his potentizing glass one minim of the substance he intends to potentize, and then adds about 100 minims of water and shakes the mixture about a minute. "My object in doing so," he says, "is to impregnate the interior of the glass thoroughly with the medicinal substance." He adds: "There is good reason to believe that once the glass becomes thoroughly penetrated with the medicine, and suppose the process of attenuation to go on forever, not only would it be impossible to wash out the medicinal properties from the glass by means of cold water, but its therapeutic power would be greatly heightened. If it were possible for Niagara to pour its unceasing torrent of mighty waters for twenty thousand years through the glass once thoroughly impregnated with the medicine, every drop in it, and each drop which has passed through it, would still correspond with the original pathogenesis of the drug, and probably much more."

Dr. Skinner says he removes one kind of medicine from the glass before potentizing another by warming his little potentizer as he holds it in his hand over a small spirit flame. It would be very wrong, of course, to ask such an impious question as, "How is it possible to vaporize metallic platinum by holding it in a vessel which would melt at much lower temperature than the platinum?"

Dr. Burdick and Dr. Deschere, of New York, have each invented a potentizer which empties at each rise in potency. Dr. Lilienthal (*North American Journal of Homœopathy*) proposes to make the thirtieth and two hundredth with a vial, a pitcher of water, a hired man, and a spittoon or slop-jar!

This brief account of the departure from pure Hahnemannian homœopathic pharmacy would not be complete without mention of Dr. Hering's statement that one globule of the 30th potency makes, with the cubic inch of air in the bottle where it lies, a new potency! "The whole air of a room," he says, "must also be penetrated by the power of the globule and become a potency, if the right proportion existed between them; but as the proportion of the air is in excess it does not become a potency!"

Of all the innovations and pretended improvements in Hahne-

mann's method of attenuation, none of the foregoing merit approval, for the reason that they fail in purity or accuracy or both.

On the ground of expense there seems to be little demand for the invention of labor-saving machines. When a supply sufficient to last a hundred years, of any remedy, attenuated by hand to the *thirtieth* or *two hundredth*, can be purchased of a reliable pharmacy for a few cents, there is no need of elaborate and costly machines to make inferior preparations.

While the deluded followers of Fincke and Swan were using their so-called 40,000ths, 100,000ths, and 1,000,000ths under the representation that they were more attenuated than the Hahnemannian preparations, they thought they performed wonderful cures, which could not be wrought with the regular attenuations. Their cry was, "Higher! higher!" When now the rottenness of these pretensions is disclosed, and it is shown that the fluxion potencies bear no comparison in tenuity with the centesimal, that they are uncertain in strength and rank with impurity, the hot-headed, hare-brained dupes are too idiotic to see their error and still cry, "Higher! higher! every year higher!"

In concluding this necessarily discursive paper, I must do honor to the genius of Samuel Hahnemann by recommending a free and intelligent adherence to the principles and methods which he introduced, and by advising the cautious acceptance of such modifications as the advance of sound science demands.

THE LIMITS OF DRUG ATTENUATION;

OR, PROOFS OF DRUG PRESENCE IN ATTENUATIONS
ABOVE THE THIRD DECIMAL, FROM THE
STANDPOINT OF THE SCIENTIST.

BY S. A. JONES, M.D., MICHIGAN.

1. CHEMICAL DIVISIBILITY.

THE starting-point of the research upon which I have the honor to report is found in Faraday's *Bakerian Lecture for 1856*—"Experimental Relations of Gold (and other Metals) to Light."*

Faraday dissolved gold in aqua regia, and reduced it therefrom with a solution of Phosphorus in Sulphide of carbon, obtaining thereby "ruby and amethystine fluids." He says, these fluids, "when in their finest state, often remain unchanged for many months, and have all the appearance of solutions. But they are never such, containing in fact no dissolved, but only diffused gold. The particles are easily rendered evident, by gathering the rays of the sun (or a lamp) into a cone by a lens, and sending the part of the cone near the focus into the fluid; the cone becomes visible, and though the illuminated particles cannot be distinguished because of their minuteness, yet the light they reflect is golden in character, and seem to be abundant in proportion to the quantity of solid gold present. Portions of fluid so dilute as to show no trace of gold, by color or appearance, can have the presence of the diffused solid particles rendered evident by the sun in this way."

* Philosophical Transactions, p. 145, 1857.

The minutest divisibility of gold recorded by Faraday, in his researches, is "one volume of gold in about 750,600 volumes of water."

The highest magnifying power used by Faraday, in the microscopical examination of these fluids, is 700 diameters.

The writer made similar "ruby and amethystine fluids" by reducing the dissolved amic chloride with an ethereal solution of Phosphorus, and could find no appearance of "grained structure," when examining these fluids with a power of 2700 diameters. If, however, one drop of such a solution be evaporated on a glass "slide," particles of gold are readily discernible with a power of 125 diameters.

Beyond all cavil Faraday's "ruby and amethystine fluids" contain gold in division, not in solution, and in particles so minute as to defy the utmost power of the microscope.

2. THE COLOR TEST.

Having found by evaporating one drop of the amethystine fluid, that the color is due to the presence of gold, it follows that the existence of color enables the naked eye to discern the presence of gold in such a solution, however attenuated, as still retains color.

Faraday's limit was $\frac{1}{750600}$ th part of gold—that is, the equivalent of our 5.756 decimal attenuation.

To determine the limit of the naked eye color-test, with such apparatus as was at hand, namely, a 15-inch long litre jar, the following experiment was made:

Took 100 milligrammes of gold (precipitated by FeSO_4), dissolved in nitro-hydrochloric acid, and evaporated to dryness on a sand-bath. Dissolved the resulting amic chloride in 100 cc. of distilled water: having then the "3d decimal attenuation" of gold in the form of a chloride (3^x).*

* This experiment is only approximately accurate. In all the nitro-hydrochloric solutions of gold which I have evaporated on a sand-bath, to drive off the excess of chlorine, I find a trace of reduced gold. There is, then, less gold than is represented by the figures given in the attenuations made therefrom. As the subject of the *mechanical divisibility* (a far more arduous research) required more time than could well be taken from more pressing duties, this trifling error remains unadjusted.—S. A. J.

Took 1 cc. of said "3d decimal attenuation," added 99 cc. of distilled water and a few drops on an ethereal solution of Phosphorus (5^x). Result: a fluid of an amethystine color.

Put the above 100 cc. of amethystine fluid in a 1000 cc. graduated jar, and added 900 cc. of distilled water (6^x). Result: a faint amethystine hue still evident.

Took of this 6^x attenuation 100 cc., put it into a fresh jar and added 400 cc. of distilled water (6.5^x). Result: on looking through the fluid, in the long axis of the jar, and with white paper underneath, the eye still sees that it is not absolutely colorless.

This is deemed the limit of the naked eye color-test, with a 15-inch column of fluid: 1 part of gold in 5,000,000 of water.

To the above 500 cc. of 6.5^x attenuation added 100 cc. of distilled water, so that the measuring jar contained 100 cc. of gold solution (?) and 500 cc. of distilled water. Then added 400 cc. of 6^x gold solution (?) (6.1^x). Result: color plainly evident—a faint amethystine tinge.

This jar now contained gold 1 part, water 2,000,000 parts. Added a few drops of stannous Chloride (SnCl_2) and set aside.

In twenty-four hours this jar contained a sediment of a royal purple color—a cloudy or flocculent, not a solid precipitate. From this precipitate, and by evaporation, was obtained the gold which is mounted on slide "A," accompanying this paper.

In the vial marked "B," accompanying this paper, is some of the amethystine fluid containing gold in suspension, and made from that reclaimed precipitate of 1 part of gold in 2,000,000 parts of water.

3. MECHANICAL DIVISIBILITY.

The presence of a metal in a trituration can be demonstrated microscopically, *with high powers*, only when the observer uses reflected light. For this purpose I used Professor Hamilton L. Smith's Illuminator in 1867, and the identical instrument was employed in the following researches.

By its use the metallic gleam of gold is seen; by transmitted light all metals look opaque and color does not aid in distinguishing them.

After trying many methods of mounting I am best satisfied with the following: Dissolve a little of the trituration in distilled water on a slide over a spirit-lamp. All trace of the crystalline structure of the sugar of milk is lost: only a transparent homogeneous film remains on the slide. Mount in Deane's gelatine (as it contains sufficient water to prevent crystallization); fasten the cover carefully with Bell's cement; finish with asphalt varnish, and throw on a ring or two of zinc cement.

Only by thus dissolving the trituration on the slide can we be sure that the most minute particles in that trituration are retained.

A mounting of the 9^x trituration of gold, and one of plain and pure Sugar of milk shall present appearances so similar under the illuminator as to make the diagnosis of real gold from "bogus" a matter of absolute uncertainty, *so far as my resources for illumination are concerned.*

Having found what was deemed to be gold in a certain trituration, specimens of the same trituration were treated under the microscope with Sulphuric acid, with Nitric acid, and with Nitro-hydrochloric acid. If the gleaming "sparks" withstood the Sulphuric acid and the Nitric, still shining through this cleansing bath, and if they disappeared in the Nitro-muriatic acid, forming bubbles of (hydrogen) gas, they were held to *be gold.*

The slide marked "B," accompanying this paper, shows gold in the 12^x trituration, with a power of 125 diameters. The vanishing-point is not reached in the 12^x trituration.

I will report upon higher triturations elsewhere, and at a future time—not having them to examine now.

THE OBJECTS AND METHODS OF HAHNEMANN IN ATTENUATING MEDICINES.

BY LEWIS SHERMAN, M.D., MILWAUKEE, WIS.

I. OBJECTS.

THE world is indebted to the genius of Samuel Hahnemann for the development of the law of cure by the similar remedy.

With the development of this law peculiar doctrines have appeared which have been supposed to be its postulates and corollaries. Notable among these doctrines are:

- a.* The primary and secondary actions of medicines.
- b.* The homœopathic medicinal aggravation.
- c.* The single remedy.
- d.* The diminution of the dose.

There have appeared also certain heresies. Notable among these are:

- a.* Itch pathology.
- b.* Isopathy.
- c.* Keynote selection.
- d.* Dynamization by succussion.

The doctrine of the diminution of the dose and the heresy of dynamization are intimately connected with the subject of attenuation.

From the time of his announcement of the homœopathic law of cure in 1796 to the period of his invention of the psora theory (1825 to 1828), Hahnemann's professed object in the attenuation of remedies was to *diminish* medicinal action, and thus avoid unnecessary medicinal aggravation. He held that the diseased organism is morbidly sensitive to the action of the homœopathic

remedy, and that therefore the dose must be reduced in strength. He taught, however, from first to last that the dose must not be reduced below the point at which it is capable of producing medicinal aggravation.

The following quotations from Hahnemann's writings, from the period 1796-1825, will be seen to substantiate these statements:

In the essay on "Scarlet Fever" (1801) he says that the $\frac{1}{432000}$ of a grain of extract of *Belladonna* is "too large a dose." The $\frac{1}{24000000}$ of a grain is recommended instead. The object of this dilution was to diminish the power of the medicine; for, he remarks, "We will sometimes meet with children who possess naturally such timorous, tranquil dispositions that in them the dose above indicated ($\frac{1}{24000000}$, gr. to $\frac{3}{15000000}$, gr.) for children of their age will not suffice to protect them from scarlet fever. The physician may, therefore, be allowed to increase it somewhat, etc."

In *The Medicine of Experiences*, Berlin, 1805, he says: "None but the careful observer can have any idea of the height to which the sensitiveness of the body to medicinal irritations is increased in a state of disease. It exceeds all belief when the disease has attained a great intensity. An insensible, prostrated, comatose, typhoid patient, unaroused by any shaking, deaf to all calling, will be rapidly restored to consciousness by the smallest dose of *Opium*, were it a million times smaller than any mortal yet prescribed. The sensitiveness of the diseased body to medicinal irritations increases to such a degree that, powers commence to act on and excite it whose very existence has been denied, because they manifest no action on healthy, robust bodies, nor in many diseases for which they are not suited."

In the *Spirit of the Homœopathic Doctrine of Medicine*, 1813, he says: "The human organism when it is diseased is, beyond comparison, more affectable by homœopathic medicine than by any other."

During this period he held that the processes of attenuation diminished the power of the medicine, but that this diminution was partly compensated for by the sensitiveness of the diseased organism to the homœopathic remedy. The ratio of diminution

of medicinal power he states to be about *four* for each degree of the centesimal scale; that is, the 2d dilution would be one-fourth as strong as the first; the 3d dilution one-fourth as strong as the second, and so on.

Following this ratio the 30th dilution would produce one-quintillionth ($\frac{1}{1000000000000000000000000000000}$ th), as much effect as the strong tincture in the same dose.

It is true that Hahnemann in this period asserts that, a given quantity of medicinal substance will produce more effect in a diluted than in an undiluted form; but he attributes this increased effect to the presentation of a greater number of points of contact with living tissue; and he expressly states that one-tenth the quantity will produce a little more than one-half the effect. *Organon*, all the editions.

In the year 1825, the seventy-first of his age, the forty-sixth after his graduation, the twenty-ninth of his homœopathic practice, the fourth after the injunction forbidding him to dispense his own medicines, two years before the formal announcement of his absurd itch, clap, and pox theory of chronic diseases, Hahnemann propounded the theory of dynamization. The statement of this theory is made in the preface to the fourth volume of the *Materia Medica Pura* in the following language:

“By the processes of succussion and trituration there ensues not only the most intimate mixture, but at the same time such a great and hitherto unknown, undreamed of change, by the development and liberation of the dynamic powers of the medicines as to excite our astonishment.

“By means of the processes of shaking and trituration we not only succeed in impregnating gradually and most intimately every particle of *unmedicinal substance* with the *power* of the medicine, but also in *developing* that power to an almost boundless extent.

“The fact that the inmost power of a medicinal agent can be infinitely developed by trituration and succussion had never been known heretofore. I have been the first to discover the great fact that the power of liquid medicines can be developed by *succussion*, and that of dry substances by *trituration*, to such an ex-

tent that substances which, in their natural crude form, did not seem to have any power, acquire an astonishing medicinal power.

"After triturating one grain of Gold with ninety-nine grains of Sugar of milk, and continuing this trituration up to the twelfth potency, the remedial virtues of Gold become roused to such an extent that a man who was impelled by intolerable anguish to take his life needs but to *smell* for a few moments of such a preparation of Gold in order to recover his cheerfulness and love of life.

"It is self-evident that in proportion as the power of medicinal substances is developed by trituration they ought to be administered in smaller doses."

In the preface to the sixth volume of the *Materia Medica Pura* (1827) he says:

"If we wish to attenuate a drop of Sundew (*Drosera rotundifolia*) to the decillionth (30th), but shake each of the bottles with twenty or more succussions from a powerful arm, in the hand of which the bottle is held; in that case this medicine, which I have discovered to be the specific remedy for the frightful epidemic whooping-cough of children, will have become so powerful in the fifteenth attenuation (spiritualization) that a drop of it given in a teaspoonful of water would endanger the life of such a child; whereas, if each dilution-bottle were shaken but twice (with two shakes of the arm) and prepared in this manner up to the decillionth attenuation, a sugar globule, of the size of a poppy-seed, moistened with the last attenuation, cures this terrible disease with this single dose without endangering the health of the child in the slightest degree."

In the preface to *Thuja*, in the fifth volume of *Materia Medica Pura*, 1828, he says that the higher dilutions of this medicine, *e. g.*, the 30th, or even the 60th, if each dilution be shaken ten or more times, so far from being inferior in strength to the lower dilutions, they are actually more powerful; consequently he recommends that each dilution be prepared with only *two* succussions.

"I dissolved," says he, "a grain of *Soda* in an ounce of water mixed with alcohol in a vial which was thereby half full, and shook this solution continuously for half an hour, and this was, in dynamization and energy, equal to the 30th development of

potency." This shows that succussion and not attenuation was the essential feature of dynamization.

In the last edition of the *Organon* (1833), Hahnemann recommended that all provings on the healthy should be made with the 30th attenuation, on the ground that the lower attenuations would not "disclose the same wealth of symptoms."

A remarkable feature of this theory of dynamization is that, by the process of trituration and succussion, the medicinal substance *loses its chemical properties*. According to the discoverer, a globule of *Phosphorus* 30th, in a powder of milk sugar, which may have been prepared for a year or more, will at the end of that time undergo no alteration from the action of air, but will still act perfectly as *Phosphorus* and not as *Phosphoric acid*. "A globule of the 30th potency contained in a vial, prepared twenty years before, still continues to possess its power undiminished, and may be used with perfect confidence for the cure of disease."

Now we know that *Phosphorus* in the tincture, the 3d, the 4th, or the 5th decimal dilution, when put on pellets and exposed to the action of the air, becomes converted into *Phosphoric acid* by reason of its strong affinity with the oxygen of the atmosphere.

These assertions of Hahnemann clearly demonstrate that a dynamized medicine, though deprived of its power to act on dead matter, still retains in an exalted degree its power to affect the human organism. This explains how our high-potency friends get the effects of Nitric acid which has been diluted in *alcohol* and left for years in contact with *sugar*!

I think I have made it clear that Hahnemann's object in attenuating medicines was not the same in the early and middle part of his life as it was in his declining years. In the latter period he had in view, the transference of the properties of the medicine from the medicinal substance to the alcohol, or the sugar of milk, the separation of the *qualities* of matter from the substance in which they inhered, or, to use his own words, "the spiritualization of the dynamic property," the unveiling and vivifying of the medicinal spirit, the increase and exaltation of the strength of the medicine, in order that it might cause disease in the healthy and cure the sick, when undynamized medicine would fail for want of power.

As in the promulgation of the psoric, syphilitic and psycosic theories of chronic diseases as the basis of treatment, the illustrious founder of homœopathy overthrew the fundamental principles of his grand system, so, in the theory of dynamization, he renounced the teaching and experience of the thirty years of his vigorous manhood, for the visionary delusions of decrepit old age. I say "visionary," because he never performed a single experiment to demonstrate the truth of his theory, nor offered a single fact in corroboration of his rash, dogmatic assertions; "decrepit," because, in still later years, he renounced his succussion doctrine, and no longer fearing the terrible consequences of too much dynamization, recklessly went to shaking as vigorously as ever, declaring that all the shaking in the world would not dynamize an undiluted substance. This is shown by the fact that he retained, in the fifth edition of the *Organon* (1833), the passage in which he gives the arithmetical ratios of diminution of strength by attenuation.

The real object in attenuation is, in case of liquids and easily soluble solids, to diminish the force of drug action; in case of solids not readily soluble, to promote absorption and to increase the number of points of contact.

II. METHODS.

Hahnemann's method of attenuating liquid medicines to the 30th degree was as follows:

Take thirty new, clean vials, holding each about half an ounce; fit them with good new, clean corks; label them all with the name of the medicine to be attenuated; number the vials by marking on the corks and on the labels the numbers from one to thirty, inclusive; put into vial number *one* as much of the strong tincture as will represent a drop or grain of the original substance; add as many drops of alcohol as with the quantity of medicinal tincture used will make one hundred drops of liquid; cork the bottle and shake it with ten jerks of the arm.

This constitutes the first dilution or development of power.

Put one drop of this first dilution into vial number *two*, and add ninety-nine drops of alcohol ; shake as before.

This constitutes the second dilution, or second development of power.

The succeeding dilutions up to the thirtieth were made in the same manner as the second, each from the next lower. The alcohol must be of the purest kind.

In the attenuation of insoluble substances the first three degrees were made by trituration, as follows :

Take a clean porcelain mortar and pestle with roughened grinding surface ; put into it thirty-three grains of sugar of milk ; add one grain of the medicinal substance ; stir for a short time with a clean ivory spatula ; triturate strongly for six minutes ; scrape off the particles which adhere to the mortar and pestle, and stir for four minutes ; triturate again for six minutes ; scrape and stir for four minutes ; add thirty-three grains of sugar of milk and repeat the process of triturating, scraping and stirring for twenty minutes ; add again thirty-three grains of sugar of milk, and repeat again the process of triturating, scraping and stirring for twenty minutes.

This constitutes the 1st trituration or the 100th development of power.

Treat one grain of this 1st trituration in the same manner with ninety-nine grains of sugar of milk.

This constitutes the 2d trituration, or the 10,000th development of power.

Treat one grain of this 2d trituration with ninety-nine grains of fresh sugar of milk in the same manner.

This constitutes the 3d trituration, or the 1,000,000th development of power.

Hahnemann affirmed that any substance brought to this degree of attenuation is rendered soluble in alcohol and water.

The 4th attenuation was prepared by adding to one grain of the 3d trituration, in a vial, fifty drops of distilled water, aiding the solution of the sugar of milk by a few turns of the bottle on its axis, adding fifty drops of alcohol and shaking with ten jerks of the arm.

The 5th and succeeding degrees of attenuation to the 30th are

made by adding to one drop of the next lower attenuation ninety-nine drops of alcohol and shaking with ten jerks of the arm.

Such is the method of Hahnemann. It is remarkable for its regularity, neatness, and accuracy, as well as for the scrupulous care he inculcates to avoid the introduction of foreign substances whose presence might modify the action of the extremely minute doses he prescribes.

THE DOSE AND DEGREE OF ATTENUATION.

BY C. WESSELHÖFT, M.D., BOSTON.

PRIOR to the last edition of the *Organon* Hahnemann seems to have sought to establish no general limit to attenuations, but in his last edition (1833) he recommended the 30th centesimal as the universal limit, which, for the sake of uniformity, it would be best to adopt. In a footnote to § 287, and in the later edition of the *Chronic Diseases*, he sanctions the use of much higher attenuations. These "high potencies" were upheld in Germany by Gross (*H. Z.*, vol. No. 6), who adduced the 1500th attenuation as proof that the limit had not been reached. Korsakoff, a Russian general and an enthusiastic layman, introduced certain methods of producing higher attenuations. (*Archiv*, vols. xi and xii.) Hahnemann furnishes an appendix, in which he expressed his mistrust of Korsakoff's method, which consisted chiefly in the preparation of what were subsequently called dry contact potencies.

Hering about that time, in the *Archiv* (vol. xiii), advocated proving with the 30th potency, and afterwards exerted his influence in favor of high potencies, as prepared by Jenichen, who introduced them about the year 1840. The latter claimed to have reached the 100th, and later the 1600th potency. He was ardently defended by Drs. Stapf and Gross. Jenichen died in 1849.

In this country the question of high potencies was not much agitated until about the year 1857, when Lehrmann's preparations, carried up to the 200th degree, were introduced chiefly by Dr. Dunham, who derived his inspiration from Bönninghausen, in Münster.

Since that time Dr. B. Fincke,* of Brooklyn, N. Y., proclaimed his high potencies to the world, claiming to have reached the 100,000th attenuation or more, and found no inconsiderable number of adherents.

These preparations subsequently proved to have been made by means of an ingenious patent contrivance, by which a medicine can be diluted to an extreme degree in a short time.

Others, finding that such preparations promised to enjoy a still greater degree of popularity, invented independent methods of preparing "high potencies." An extremely ingenious little mechanism for such a purpose was constructed by Mr. Boericke, of Philadelphia. This apparatus is so constructed that a large quantity of alcohol is brought in contact with a drop of medicine and subjected to a great amount of succussion.

Since that time Dr. Samuel Swan, of New York, has also prepared high potencies, claiming that he has reached the 1,000,000th centesimal. His apparatus forcibly throws a stream of water into the potentizing glass.

The first volume of the new English periodical called the *Organon* contains a description of an apparatus called the "Centesimal fluxion potenziizer," invented by Dr. Skinner, of Liverpool. This registers potencies up to the 1,000,000th.

Still more recently Dr. Martin Deschere has invented and described another potentizing apparatus.

Such, in brief, are the successive steps by which homœopathists have advanced in the scale of attenuation. It is an interesting phenomenon that the dose formed in Hahnemann's time as well as now, in the minds of many, is the test of genuine homœopathic belief and practice.

The accurate proving of drugs has received but slight attention in comparison with the zeal displayed in the invention of new methods of attenuation. It seemed as if imperfectly proved medicine, unsubstantiated assertions of the superiority of highly potentized medicines, ancient traditions introduced into symptom lists in place of accurately confirmed observations, were all to be counterbalanced or annulled by increasing the attenuation, or

* On High Potencies and Homœopathies. Philadelphia: A. J. Tafel. 1865

“potency,” as it is called, and that this high degree of supposed refinement could effect the, as yet unavoidable, inaccuracies of the *Materia Medica*.

This peculiar phenomenon has now been developing for many years, without the least attempt on the part either of ardent potentizers or of their opponents to inquire into the nature of matter itself. Everything was taken for granted, or Hahnemann’s views on the subject fully and literally adopted, and unconditional compliance expected of all who call themselves homœopaths. Thus it is demanded of us to adopt without other than clinical tests Hahnemann’s views, that insolubles become soluble above the 3d centesimal, and that soluble substances are infinitely divisible.

Thus far all negative results have been absolutely disregarded and excluded. Even if they should prove to be less frequent than the positive, their weight as an integral part of statistical evidence would necessarily change the aspect and interpretation of our clinical results, furnishing fewer but more reliable data.

Whether originating with Hahnemann or not, the method of dilution for the purposes of attenuation, has been of such therapeutical value, and has been the means of working such wonderful changes in the molecular constitution of matter, that he deserves to be called the founder of molecular pathology, as well as of molecular therapeutics.

Much evidence has been lately deduced by Mayrhofer, Atto-my, J. Edward Smith, and myself, to prove that the Hahnemannian scale of attenuations has been erroneously applied to many substances, which are not soluble in alcohol and water.

We will now consider to what extent attenuation may be carried by means of dilution of soluble substances, and inquire how far the results obtained by physicists, in the last decade, agrees with the assumption of infinite divisibility by the means applied by Hahnemann and his successors in the method of attenuating medicines.

I use the word attenuation and dilution as distinct from potentization, which is not regarded as synonymous with it, as it is well known.

THE ULTIMATE PARTICLES OF MATTER.

Molecules and atoms have an existence, which is scarcely a matter of doubt to-day. Their dimensions are calculable and may be approximately demonstrated. Matter consisting of molecules has not a homogeneous, but a "grained" structure, as it is called. When matter is subdivided beyond the constitution of this grained structure, the entity of its molecules is destroyed and they are resolved into atoms of their elements. The limit where such decomposition takes place has been calculated for several substances with approximate exactitude by means of physics and mathematics.

For the data given below we are indebted to the English physicist, Sir William Thompson, and also to Professor J. Clerk Maxwell, who have based their results on the calculations of certain German mathematicians, especially those of Clausius, of Bonn, Lohnschmidt, and others.

It is not within my ability to state in detail the methods and means employed, as these experiments and calculations are based upon the higher branches of physical and mathematical science, which a much-occupied physician cannot hope to cultivate. The calculus of probabilities furnishes facts which can be merely stated in a few brief pages. It will be proper, therefore, to let experts speak for themselves.

In demonstrating the size of atoms* the following conclusions were arrived at. In order to illustrate the fact that matter is only apparently homogeneous, he cites Cauchy's law, which shows that in palpably homogeneous bodies, such as glass or water, contiguous portions are not similar when their dimensions are moderately small fractions of the length of a wave of light.† In short, the diameter of a molecule, or the distance from its centre to that of a contiguous molecule, in any transparent liquid or solid, exceeds $\frac{1}{1000000}$ th of a wave length of light, or a two hundred millionth of a centimeter.

* Size of Atoms, a lecture by Sir William Thompson, reprinted in *Nature*, March 31st, 1870, p. 557.

† See also *The New Chemistry*, by Josiah P. Cook, Jr., New York. D. Appleton & Co., 1874, p. 21, et seq.

These calculations, which permit of such measurements, are a strong support of the non-homogeneousness of matter. This is compared to brickwork, which at a distance appears to the eye homogeneous, but is in reality capable of being broken up into bricks with interspaces of mortar. Viewing a large mass composed of large particles at a distance, and another consisting of inconceivably minute particles in close proximity to the eye, has the analogous effect of a homogeneous appearance. Long before this, Cauchy, Newton, and after him, La Place, calculated the velocity of sound very accurately, on the theory that matter (air) consists of molecules. The exactness of these calculations, both as regards the nature of light and the velocity of sound, furnish strong evidence of the molecular nature of air, and admit even of a calculation of the dimensions of its ultimate particles.

But Sir William Thompson has added evidence in this direction from three other sources, which, though well known, deserve brief mention here. In producing contact electricity by approximating zinc and copper plates, it is not the thickness of the plates which augments their force but their number. The amount of force developed, that is work done, by the pile an inch in thickness, will be greater and greater as there are more plates added. Therefore it is only necessary to make the plates thinner and thinner, and more numerous in proportion, to get more work done by the same amount of mass, till at last force enough is obtained in the form of heat to melt the whole pile of zinc and copper. Or, when these shall have reached a thinness of a four-hundred millionth of a centimeter, the work and heat equivalent will be increased 990 times. This is more than is required to melt the whole mass, that is, to form a chemical combination. Hence, plates of zinc and copper of 300,000,000 centimeters in thickness, placed close together, approximate the attenuation required for a chemical combination, that is, to the diameter of a molecule.

These figures, at the same time, agree very well with Cauchy's calculation of molecular distances and lengths of waves of light.

In the *Proceedings* of the Royal Society for April, 1858, is an article by the same eminent author, "On the Thermal Effects of Drawing Out a Film of Liquid." These illustrations throw much

light on the size of ultimate particles of matter, which conclusions are reached by the following deduction from given facts:

It is proved (from the second law of thermo-dynamics) that about half as much more energy in the shape of heat must be given to the film to prevent it from sinking in temperature while it is being drawn out into a soap-bubble. Hence the intrinsic energy of a mass of water in the shape of a film kept at constant temperature, increases by twenty-four milligram-millimeters for every square millimeter *added to the area*. For the work done in stretching a water film to any degree of thinness reckoned in millimeter-milligrams is equal to sixteen times the number of square millimeters by which the area is augmented, provided the film is not made so thin that there is any sensible diminution of contractile force.

THE CONTRACTILE FORCE RECKONED.

Suppose a film to be given with a thickness of a millimeter, and suppose its area to be augmented ten thousand and one fold, the work done per square millimeter of the original film would be 240,000 millimeter-milligrams. The heat equivalent of this is more than one-half of a degree Centigrade of elevation of temperature of the substance. The thickness to which the film is reduced in this supposition is very approximately a ten thousandth of a millimeter, while there is yet no sensible diminution of contractile force.

But now suppose the film to be stretched to a twenty millionth of a millimeter, the work done would be more than 2000 times greater than first calculated. The heat equivalent would be 130 times the quantity required to raise the temperature of the film one degree Centigrade. A much smaller quantity of work (*i. e.*, heat) would convert it into vapor long before it reached this point.

The conclusion is unavoidable that a film of water falls off greatly in contractile force before it is reduced to a thickness of a twenty millionth of a millimeter. Hence it is scarcely possible, upon any conceivable molecular theory, that there can be any considerable falling off of contractile force, as long as there are

several molecules in thickness. It is, therefore, nearly evident that there are not several molecules in the thickness of a twenty millionth of a millimeter of water.

Sir W. Thompson furnishes other valuable evidence with regard to the size of molecules and their numbers in given volumes of matter. These calculations were partly dependent on the investigations of others, but as Professor J. Clerk Maxwell is mentioned as the chief authority in regard to deductions from the kinetic energy of gases, I will quote his conclusions, so far as they relate to the subject before us.

At this period it will be well to define, what was taken for granted in quotations from the previous calculations, namely, the terms molecule and atom have been used somewhat indiscriminately, but in modern times they are defined and their definitions are universally accepted.

An atom, says Maxwell,* is a body which cannot be cut in two. A molecule is the possible portion of a particular substance. If any portion of a molecule were removed, it would no longer be able, along with an assemblage of other molecules similarly treated, to make up a mass of the original substance.

Every substance, simple or compound, has its own molecules. If these molecules be divided, their parts are molecules of a different substance. They become the substances of which the molecules are constituted. An atom, then, must be a molecule of an elementary substance. Every molecule is *not* an atom, but every atom is a molecule.

A molecule of common salt, for instance, if divided, will yield one atom of chlorine, and one of sodium.

It was formerly believed that the molecules of all bodies are in motion, and this is not only held to be the case now, but accurate data underlying the most exact application of physics and chemistry are derived from the study of such motions.

In solids the motion of molecules is imperceptible, while in liquids and gases, they are not confined to definite limits, but work their way through the whole mass, as has been proved.

* Molecules. Lecture delivered before British Association, at Bradford, by Professor J. Clerk Maxwell. *Nature*, vol. viii, p. 437 et seq. September, 1873.

It is now well understood that the motion of flying molecules is the cause of the pressure of gases. This principle was first stated by Bernoulli, and again by Lasage of Geneva. The latter endeavored to explain with this principle the law of gravitation, but his theory has not been accepted. Later, Herapath, in his *Mathematical Physics*, 1847, and Dr. Joule furnished evidence upon the subject. Professor Clausius of Bonn, however, was the first to give precision to the subject of the kinetic laws of molecular motion in gases. It is to him that we owe, to a very large extent, what has been accomplished, for he not only found a formula, but also conceived a new dynamic idea, by which we are enabled to establish several important conclusions, without much symbolical calculation.*

It would exceed the limits of this article to introduce the whole evidence in relation to this vast subject. Those who would follow out the mathematical calculations of Professor Clausius, upon which Professor Maxwell based his, are referred to the article cited.

If the velocity of molecules is given, and the number varied, the pressure in a vessel of given size is, therefore, proportional to the number of molecules, that is, to the quantity of gas in it. This explains the fact, discovered by Robert Boyle, that the pressure of air is proportional to its density. The same applies to mixed gases, each of whose molecules moving about and colliding with the others, will have the same energy of motion, as shown by J. Clerk Maxwell, and further substantiated by Dr. Ludwig Boltzmann. According to these researches, a cubic centimeter of every gas, at standard temperature and pressure, contains the same number of molecules (Gay-Lussac's *Law of Equivalent Volume of Gases*). The next step was the calculation of the actual velocity of a molecule of hydrogen. This was first done by Dr. Joule. This done, the exact distance a molecule travels before striking another could be determined by experiments on the diffusion of the gases; Professor Clausius calls this distance the mean path of a molecule.

* On the Dynamical Evidence of the Molecular Constitution of Bodies. Lecture, delivered at the Chemical Society, February, 1875, by Professor J. Clerk Maxwell. *Nature*, vol. ii, p. 356.

The rationale of this calculation is best described by Sir W. Thompson in the lecture quoted above. We may regard it as an established truth of science, says this author, that a gas consists of moving molecules. The mean length of proximately rectilinear portions of path, of each molecule, is many times greater than the average distance from the centre of each molecule to the centre of the molecule *nearest* it at any time. Clausius proves by simple application of the calculus of probabilities that the average length of the free path of a particle, from collision to collision, bears to the diameter of each globe (molecule) the ratio of the whole space, in which the globes move, to eight times the sum of the volumes of the globes. It follows that the number of the globes, in unit volume, is equal to the square of this ratio divided by the volume of a sphere whose radius is equal to that average length of free path.

From the calculations of Joule, Maxwell, and Clausius, the average velocity of the molecules of oxygen, nitrogen, or common air, at ordinary temperature and pressure, is about 50,000 centimeters per second; and the average time from collision to collision, the five-thousand-millionth of a second. Hence, the average length of path of each molecule, between collisions, is about one-ten-thousandth of a centimeter. Now, according to the theorem of Clausius, the average length of this path cannot be more than five thousand times the diameter of the gaseous molecule, and the number of molecules in a unit of volume cannot exceed 25,000,000, divided by the volume of a globe, whose radius is that average length of path. Taking now the preceding estimate, one-one-hundred-thousandth of a centimeter, for the average length of path, we conclude that the diameter of the gaseous molecule cannot be less than one-five-hundred-millionth of a centimeter; nor the number of molecules in a cubic centimeter of the gas (at ordinary density) greater than 6×10^{21} , or six thousand million million.

These calculations apply to gases; but from these the molecular constitution of fluids can also be deduced, as the following statements will clearly demonstrate:

The densities of known liquids and solids are from five hundred to sixteen thousand times that of atmospheric air, at ordinary

pressure and temperature; hence, the number of molecules in a cubic centimeter may be from 3×10^{24} to 10^{26} (three million, million, million, million, to one hundred million, million, million, million). In a cubic arrangement of molecules the distance from centre to nearest centre in solids and liquids may be estimated at from one-one-hundred-and-forty-millionth to one-four-hundred and-sixty-millionth of a centimeter. These lines of argument establish with a high degree of probability the conclusion that, in any ordinary liquid, transparent, solid, or seemingly opaque solid, the mean distance between centres of contiguous molecules is less than the hundred-millionth, and greater than the two-thousand-millionth of a centimeter.

Turning once more to Professor Maxwell's statement, we find the following additional confirmation of the preceding conclusions:

The resistance of liquids to compression makes it probable that their molecules must be about the same distance from each other as two molecules are when they encounter each other in a gas. This subject was tested by Lorenz Meyer, who compared the densities of different liquids with the calculated relative densities of the molecules of their vapors, and found a remarkable correspondence.

Lohschmidt deduced from the dynamical theory the following remarkable proportion:

As the volume of gas is to the combined volume of molecules contained in it, so is the mean path of a molecule to one-eighth of the diameter of a molecule.

In this way Lohschmidt, in 1865, first calculated the diameter of a molecule. Independently of him and of each other, Mr. Stoney, in 1868, and Sir W. Thompson, in 1870, published similar results, as we have seen.

According to the table which Maxwell has calculated from Lohschmidt's data, the size of the molecules of hydrogen is such that about two million of them in a row would occupy a millimeter, and a million, million, million, million (10^{24}) would weigh between four and five grams.

In a cubic centimeter of any gas, at standard pressure and temperature, there are about nineteen million, million, million

molecules. As we have seen, Sir W. Thompson has calculated this number at six thousand million, million, millions, so that we have a difference between nineteen and six thousand in the calculation. Although it has no direct bearing on the conclusion we may have to draw from these calculations, the reader will desire to know the general conclusion arrived at by physicists. In this connection it is interesting to know that Mr. H. C. Sorby* has compared the calculations of Messrs. Stoney, Thompson, and Maxwell. Each of these observers determined the number of ultimate atoms in a given volume of any permanent and perfect gas, and reduced their results to an average of 6,000,000,000,000 molecules in a cubic $\frac{1}{100}$ of an inch.

There is no question that more numerous calculations will bring this average much nearer the truth. But, as Mr. Sorby informs us, Professor Clerk Maxwell's calculations are based on more correct data than those of the other observers, we will in our future deductions use these as a standard.

EFFECTS OF ATTENUATION OR DILUTION.

We have now arrived at the knowledge of certain data with regard to the constitution of matter, which we may apply to the hypotheses entertained regarding the result of attenuation, potentiation, or dynamization of medicines, for so long a time a subject of contention in the school of Hahnemann; and, although this question has been avowedly secondary and subservient to that of the law of similars, it has practically been made the standard by which members of our school measure each other. It also constitutes the most salient point for the aggressions of the old school.

The method of attenuation by the progressive dilution or division of a given drop of fluid by one hundred, has been above described. It might also be defined as progressive multiplication of the denominator of the fraction of $\frac{1}{100}$ by the power of 100. This process has been repeated in modern methods of potentiation, a million times and more.

Thus to furnish a few arithmetical ocular demonstrations of

* H. C. Sorby, F.R.S., Anniversary Address as President of the Royal Microscopical Society, from American Laboratory.

what is actually reached by this method of subdivision, we would have in the 1st centesimal attenuation $\frac{1}{100}$, in the 5th, $\frac{1}{100000000}$, in the 10th, $\frac{1}{100000000000000000000}$, etc.

In the 20th we would have the ten thousand sextillionth of the original drop or grain;* in the 30th, a fraction of the original medicine represented by 1 as the numerator, and 1 with sixty noughts behind it as the denominator, for which we hardly have room on ordinary paper. It is, therefore, conveniently expressed in modern mathematics by $\frac{1}{10^{60}}$. The exponent 60 divided by 2 gives the number of the attenuation reached; divided by 6, the number of millionths.

Sir W. Thompson furnishes us with the proportions which a molecule according to his figures bears to the diameter of the earth. "To form some idea of the coarse-grainedness indicated by this conclusion, imagine a rain-drop or globe of glass as large as a pea to be magnified to the size of the earth; each constituent molecule being magnified in the same proportion, the magnified structure would be coarser-grained than a heap of small shot, but probably less coarse than a heap of cricket-balls."

The difference between small shot and cricket-balls is, probably, the difference between Thompson's and Maxwell's calculation.

If, therefore, a molecule, the dimensions of which Thompson states are no less than the $\frac{1}{500000000}$ of a centimeter, were as a small shot to the earth, what would a particle of matter contained in the 30th centesimal attenuation be? It would be equal to the difference between a five-hundred-millionth of a centimeter and $\frac{1}{10^{60}}$ of a drop—in other words, a septillion times smaller.

* I cannot agree with Dr. F. Katsch (Alg. H. Z., vol. 98, No. 23), that Hahnemann committed a great error in calculating that the 30th contains the decillionth part of a drop of original medicine. The calculations of Professor Jolly regarding molecular contraction do not seriously invalidate Hahnemann's simple arithmetical problem. Although only 3000 drops of alcohol are used, nevertheless in order to dilute the *whole* original drop to the 30th, a quantity of alcohol would be required exceeding the volume of the earth 925 octillion times. In this the Academy of France is probably correct.

In order to ascertain what relation the fractions, expressing the quantity of medicinal substance contained in our attenuations, bear to Thompson's and Maxwell's molecular dimensions and weights, let us first take Maxwell's conclusion "that the volume of a substance, when reduced to the liquid form, is not much greater than the combined volume of the molecules." It follows that if a liquid represents very nearly the combined volume of its molecules, the volume of a drop of liquid would represent the volume of its molecules.

One drop of water is equal to one minim or 1.0533 of a grain, which we will express by a common fraction of $1\frac{1}{25}$ of a grain.

Maxwell calculates that a million, million, million, million of molecules weigh between four and five grams.

Let us say $4\frac{1}{2}$ grams = $69\frac{3}{10}$ grains, or $66\frac{9}{25}$ minims.* This represents the number of drops of water in $4\frac{1}{2}$ grams, which, as we are dealing with huge numbers of molecules, we will call by an even number, 67. Dividing with this the number of molecules (one million, million, million, million), we obtain a quotient of a little less than fifteen thousand million, million, millions, which represents the number of molecules in one drop or minim in weight, a figure of 23 places, beginning with 14925, etc.

Now this is a huge sum of particles occupying a small space. Our method of applying medicines demands that this mass of molecules should be spread over a much larger space in order that each minutest portion may have full opportunity of exerting its special and peculiar influence upon the organism. Numerous facts show us that it will exert its power less potently when administered in bulk, undiluted or unattenuated. It has been assumed that the divisibility of soluble substances and liquids is infinite, but the researches of modern times almost entirely deny this.

If the volume of a drop represents (very nearly) the volume of its molecules, then one molecule would represent the ultimate fractional part of a drop.

The foregoing simple sum in division distinctly shows that the

* One grain in weight being equal to 1.0533 minims.

11th centesimal attenuation would represent the limit to which this process may be carried according to the number of molecules in a drop of liquid of the density of water, taking Maxwell's calculation as our standard.

But let us suppose that Maxwell's calculation fell too far below the actual number of ultimate particles, and that Thompson's figures approached much nearer to the actual rate. The data from which we started were that, according to Maxwell, 10^{24} molecules of hydrogen weigh $4\frac{1}{2}$ grams or 67 minims, and that a cubic centimeter of hydrogen contains 19×10^{18} molecules, while according to Thompson a cubic centimeter of hydrogen contains 6×10^{21} molecules. Hence, any given quantity of hydrogen contains, according to Thompson, $\frac{6 \times 10^{21}}{19 \times 10^{18}}$ times as many molecules as it does according to Maxwell.

The quotient found by dividing the numerator of this fraction by its denominator is 3.158×10^3 . Now, by reducing Maxwell's proportion to Thompson's, by multiplying the above quotient by 15×10^{21} , we get 4.737×10^{23} , which would represent the number of molecules in a drop of liquid according to Thompson; that is, between 4 and 5; while we would have only between 1 and 2, if the 11th attenuation is the limit according to Maxwell. The 12th or 13th at most would be the limit of our method of attenuation, according to Thompson's calculation of the number of molecules in a drop of the density of water.

SUFFICIENCY OF ELEVENTH CENTESIMAL ATTENUATION.

The 11th centesimal dilution or attenuation potency, if you will, should be a practical limit of our method of attenuating medicines. I have already furnished illustrations of our want of practice in estimating enormous figures representing volumes or distances, and shall illustrate the subject by more examples.

The 11th centesimal dilution would somewhat exceed the number of molecules in a drop, which would be represented by a figure with 23 places, beginning with 14925, etc., or fourteen thousand nine hundred and twenty-five mill. mill. mill. To be always on the safe side, we have called this in round numbers

fifteen thousand mill. mill. mill. molecules in a drop, of which each molecule is a fractional part, expressed by the last figure as a denominator, with 1 for a numerator.

That is, our 11th attenuation has not yet reached the smallness of a molecule.

But let us proceed to the 12th centesimal; multiplying that numerator of our 11th by 100, it would immediately exceed by that proportion the limits of the size of a molecule and *numbers of molecules in a drop*. Hence this limit should satisfy the mind of even the most refined materialist.

We should establish a limit somewhere for the mind to rest upon.

It is a proof of our limited powers of conception, for want of training, to think we have not reduced matter far enough when we have reached the degree of smallness of a molecule.

A scale of attenuations reaching from 1 to 11 is enough to satisfy even those who boldly reject the agency of material influences, and candidly believe in spiritual agencies.

A drop of medicine reduced to the eleventh degree of attenuation on the centesimal scale, would be the ten-thousand-trillionth part of the original drop.

Now, according to Schimko,* the body of the sun is eight hundred times greater than all the planets taken together. Still it is much too small to form a conception of its septillionth part, which would have barely the size of 333,000th part of a poppy-seed. Now what would be the septillionth part of a drop or grain, if we cannot form a conception of such a fractional part of the sun? Ah, but we are only dealing with the eleventh. Very well, that is just about one-half of a septillion; but is it easier for us to form an adequate conception of one-half of the 333,000th part of a poppy-seed or one-half of one-ten-thousand trillionth part of the sun? This yet foils our imagination.

I have not made all these calculations; they have, however, never been disputed; neither have Thompson's nor Maxwell's calculations of the size of a molecule.

* Die homœop. heilmethode in mathematischer und chemisch-geologischer hinsicht betrachtet.—Teschén, 1829. 2d Edition.

Schimko's figures and those of the Academy of France may differ, by a number or ciphers, from those of other mathematicians, but they will not vary more than the figures of the last-named authors do from each other.

After much testimony in regard to the limits of subdivision of matter, it will be unnecessary to dwell on the attenuation above the eleventh, to say nothing of the two-hundredth and millionth. It is easy enough to reach them. A few days of labor spent with the ingenious apparatus of a Fincke, Boericke, Swan, or Skinner, will reach that number. But the centesimal scale takes such enormous strides in figures, while we, unconscious of what is progressing, look on and see only a small quantity of water and a few vials.

The authorities, quoted above, differ in their calculations by a thousand millions. The assumption of round numbers, though causing slight deviations, increases the inaccuracy on the side of safety, and it will not be difficult to determine the limits within which the inaccuracy exists, and beyond which it can no longer be of importance.

But suppose, for the sake of admission, we say the twelfth or thirteenth is the limit beyond which no molecules can be carried by the process of attenuation, any attempt to distribute molecules still farther will be useless.

It follows, further, that in several attenuations, before reaching this point, say in the eighth or ninth, the molecular particles must have been reduced very greatly in number; or in other words, the substance to be attenuated or potentized must have been reduced by the method of Hahnemann to the limits of highly rarefied gas, which condition it could only maintain so long as the disintegrated particles of matter still may be presumed to exert any influence upon each other by attraction or repulsion, throughout the medium in which they are suspended. What their influences may be, can be deduced from our present knowledge of the diffusion of gases, and also the late remarkable results obtained by MM. Caillietet and Pictet, in regard to solidification of gases. Hence, unless the most careful, conscientious, and painstaking researches and calculations are erroneous,

the limit of divisibility of soluble substances does not extend so far as had been hitherto assumed.

The 20th attenuation or potency by far exceeds all bounds of molecular distribution by dimensions so enormous that ordinary imagination can hardly realize it. Distances between the molecules of diameters of the earth would not express the degree of separation.

The above estimates have been made with regard to water. Now, most of our attenuations are made from alcohol, more or less diluted with water. But a drop of alcohol is only about one-third of that of water in size; the number of molecules in a drop of alcohol is therefore only about one-third of those of water, taking into consideration the lesser density of the former. Hence, any substances dissolved in alcohol will not bear dilution by water quite so far as watery dilutions would.

INFLUENCE ON DISTANCES BETWEEN MOLECULES.

Aside from, and independently of, the impossible diminution of size of a single molecule, another now well-known law must be considered in connection with the changes wrought in the molecular constitution of matter by extended separation of its constituent particles. It is what has been learned since Faraday's time, concerning the condensation of gases.* Without entering into the history of this process, a short statement must suffice.

The motion of molecules and their velocities have been determined, at ordinary pressure and temperature, so that according to Clausius, the molecules of common air move at the rate of 485 meters per second, those of hydrogen at the rate of 1844 meters per second.

It is now known that differences of temperature cause great differences of attraction and repulsion. Under mere mechanical pressure applied to gases, first there takes place attraction, which

* An excellent statement of the subject will be found in the Faraday Lecture, delivered before the fellows of the Chemical Society, November 12th, 1878, by Ad. Wurtz, membre de l'Institute, etc. Published in "Nature," and reprinted in "Scientific American" Supplement, 156.

reaches a maximum; then, as the pressure is increased this attraction diminishes, and at length becomes repulsion, which is only overcome by diminution of temperature. This takes the place of mechanical pressure, and may condense gases (hitherto called permanent) into solids. On this principle MM. Cailletet and Raoul Pictet have, independently of each other, succeeded in reducing oxygen and hydrogen to liquid form.

But our method of dealing with matter in order to prepare it as a medicine, is not to condense it, but to expand it, and to increase the surface of its particles. Heat does this in the case of gases, and overcomes cohesion. We endeavor to separate molecules of matter by solution and diffusion in a liquid. We do this at the ordinary temperature at which soluble substances will be dissolved, and consequently permit themselves to be separated by the interposition of the fluid used, *i. e.*, water or alcohol.

Now, in liquids the influence of cohesion is manifest; and if two liquids combine as water does with alcohol, the particles or molecules of each must become separated by overcoming the cohesion.

This cohesion would necessarily be very rapidly overcome if, *e. g.*, the alcohol were in small proportion, while that of water were constantly increased.

Supposing we have a vial, holding one cubic centimeter of alcohol, to which we add one drop of any other liquid, say a tincture; that drop of tincture will immediately be expanded to the size of one cubic centimeter plus the volume of one drop, and its particles or molecules will be separated in proportion.

And, just as gases, if expanded and their molecules set at liberty, will fly into space and cease to exert any attraction or repulsion upon each other, just so the molecules of one drop when separated by enormous spaces, will lose their influence on each other, and the distance separating them will increase in the same ratio as the molecules of a drop are diminished in number in our dilutions.

For the sake of argument, let us assume that these distances do not interfere with the action of molecules upon each other, even if the whole drops of original substance could be expanded

to fill continuous space. Still, as only a fraction of it is thus expanded, the distances do not come into play as do the walls of vials, which if they contain anything after the 12th or 13th attenuation, do so only by accident.

Whoever will take the trouble to form in his mind a conception of what is actually going on, in making dilutions by means of the centesimal scale, will perceive that the chances of retaining fractional parts (molecules) of original matter, diminish at the rate of one hundred divided by one hundred many times, thus soon lessening the chances by millions.

In the 11th dilution, as has been shown, according to the molecular constitution of matter, there can be but few left, and these, if still present, will have been separated by distances, which would be proportionate to the whole mass of alcohol or water required to attenuate the entire drop.

What these distances would be has been calculated many times, and is more tedious than difficult. Dr. Schimko also furnishes us with an idea of these distances. He calculates that in order to attenuate a drop to the 30th attenuation, it would require a globe of alcohol or water of thirty-six billions of miles in diameter, or 1,760,000 times the distance of the sun from the earth. Light travelling 41,000 miles in a second, and reaching the earth from the sun in seven seconds, would require twenty-eight years in order to traverse the diameter of such a mass of water or alcohol. Hence one drop of this enormous mass would contain precisely the quantity of medicinal substance which is contained in one drop of the 30th.

It will be easily understood that at the 11th decimal dilution, the limit of conceivable minuteness has been transcended; for to us one-third of the above enormous distance is already incomprehensible.

Von Grauvogl has demonstrated that, according to the investigations of Professor Jolly, in solutions of certain salts, molecular contraction takes place to such an extent as to show that molecular attraction is exerted through great distances of space, notwithstanding enormous preponderance of the proportion of the dissolving medium to the substance dissolved. Applying this to the 30th dilution, he claims that Hahnemann's arithmet-

ical statement, as to the quantity of original substance, contained in a fractional part (one drop) of the 30th, cannot be correct, and that it must greatly exceed the actual amount.

When we further consider the enormous distances by which we actually separate molecules, and separate them by walls of vials, Von Grauvogl's conclusion, though of weight, does not refute the simple arithmetical calculation by which we are accustomed to estimate the quantitative contents of attenuations.

LIMITS OF THE DIVISIBILITY OF MATTER.

The preceding statement is not repeated here to show the futility of Hahnemann's ingenious method of dilution and potentiation; for it was never necessary to attenuate a whole drop to the 30th, and consequently thirty little vials and about three thousand drops of alcohol would accomplish it before our eyes.

Neither would there be any objection, were matter infinitely divisible, as was properly assumed at Hahnemann's time. But the course of modern researches, with its manifold revelations, points so strongly and unmistakably to the non-homogeneousness of matter, and to its molecular constitution, that we should pause. For the present we have no alternative but to accept that position of our state of knowledge.

Were matter infinitely divisible, all mathematical calculations as to transcendent minuteness of particles, attained by attenuation, would be deprived of much force, let the particles attained by attenuation be ever so small. The septillionth part of the mass of the sun would be no larger than the three-hundred-and-thirty-three-thousandth part of a millet-seed, and the septillionth part of a grain would be outside of all possible conception.

If matter is inconceivably and infinitely divisible, such reasoning would furnish no potent argument against the process of attenuation. But if matter is limited in divisibility to measurable molecules and atoms, we should say that any further attempts at attenuation were entirely futile and the effects illusory.

While we require only a few ounces of alcohol or water to reach the 30th attenuation, the calculation of the inconceivable minuteness of a particle of medicinal matter which would be contained in a drop is nevertheless true, and exceeds the dimensions

of molecules by enormous figures, comparable only to the extreme distances and magnitude of heavenly bodies.

To reach such almost transcendental distances and dimensions it is not necessary to cite the 30th centesimal attenuation. Take only the figure known as a quadrillion, or the figure 1 followed by 24 ciphers (10^{24}). How great a volume of water is formed by a quadrillion of drops counting four drops to a cubic line? The answer is: 7017 cubic geographical miles, or 35,085 cubic English miles.*

The fraction of a drop or grain diluted to the 12th attenuation would then be a drop divided by the 35,085 English cubic miles of water or alcohol.

This is true, and cannot be destroyed by the argument that only a few ounces of alcohol and a few vials are needed. The simplicity of this argument, coupled with the vastness of progressive multiplication by the power of one hundred, leads beyond the conception of the mind, which clings only to the few ounces of visible vehicle contained in two or three dozens of little vials.

We have been treating of the molecular constitution of matter as related to the principle of attenuation. But, as the molecular theory involves the idea of the divisibility of molecules, its relation to our method of attenuation deserves notice here. Chemistry has turned the molecular theory to practical account, and by its demonstrable divisibility of molecules into atoms has remodelled chemical definitions throughout. Accordingly molecules now mean compound substances; and elementary molecules which, when divided into elementary constituents, separate into atoms †

The point which we are to consider is that we are not aiming to split molecules into atoms, because, to the best known and accepted axioms of chemistry, belongs the fact *that a molecule of compound matter broken into atoms is no longer the original substance*. A molecule of salt, of one atom of Chlorine and one

* Kriess Arithmetic.

† See Modern Chemistry, and also Ad. Wurtz's Lectures, quoted above; these furnish very clear accounts of the present state of chemical science, based on the atomic theory.

atom of Sodium, breaks into these constituents, if broken at all; the same applies to all compound bodies. We use in our dilution only soluble bodies; these soluble bodies are *compounds*. When we have attained by attenuation the limit of their molecular constitution they cease to be what they were, *e.g.*, salt, Arsenious acid, or Belladonna, etc., in tincture; hence we should cease to attempt to subdivide them farther.

For only one of two conditions can possibly occur; either they are no longer what the notation in the label would imply, or not having been subdivided into atoms, their molecules have become fewer and fewer in number, till at length at or about the 11th centesimal they cease to be present altogether. Chemistry, however, teaches that molecules consist of atoms, varying from two to two hundred and ten; the hydrogen molecules being taken as the standard, consist of two atoms (microcrits) of hydrogen.*

Now, even if it were admitted that molecules could be separated into their atoms by trituration, succussion, and dilution, the number of molecules contained in a drop multiplied by two hundred would not be equal to two degrees of potentiation by one hundred. Multiplied by a million it would only place the limit at the 13th attenuation or potency, instead of the 10th, which we have found to be the limit in the light of molecular science.

RELATIVE RELIABILITY OF DATA OF MOLECULAR SCIENCE.

It becomes, of course, a question how far the present state of knowledge concerning the molecular constitution of matter is based upon actual facts, or at least upon undisputed theories. Let us hear what the authorities above quoted have to say.

Maxwell has clearly defined the degrees of reliability of the various stages to which physical tests and mathematical calculations, as based upon the laws of Avogadro, Boyle, etc., have led. In the first rank are placed what is known of the relative masses of molecules, of different gases, and their velocities in meters per second, as obtained from pressure and from the density of gases.

In the second rank stand the relative size of molecules of dif-

* New Chemistry.

ferent gases, the length of their mean paths, and the number of collisions per second, deduced from experiments on the three kinds of diffusion. Their received values are rough approximations.

In the third rank stands the absolute mass of the molecule. Its absolute diameter and the number of molecules in a cubic centimeter are not precisely known nor even approximately conjectured. But we know the relative masses of different molecules with great accuracy, and we know their relative diameter approximately. From these we can deduce the relative densities of the molecules themselves. So far we are on firm ground.

In the application made of this conclusion to our method of attenuation and potentiation sufficient and ample allowance should be and has been made for approximate calculations, and even conjectures with regard to the absolute number of molecules in a given volume.

EXTREME ATTENUATION, OR "HIGH POTENCIES," SUPPORTED BY SPIRITISTIC REFLECTIONS.

Hahnemann himself, in the last edition of the *Organon*, and later, in the last edition of *Chronic Diseases* (vol. v, 1838), insisted that there is a distinction to be made between dilutions and potencies, or dynamizations. He places the limit of dilution just where taste and color of any substance cease.

Dynamizations are produced not only by progressive dilution, but by increased force and by the number of strokes or concussions employed. In this way, he says, we already obtain in the 50th potency medicines of the most penetrating efficacy.

Hahnemann does not distinctly state it as his conviction that matter ceases to be present at the limit of taste and color, but maintains that the latent medicinal virtues of drugs are developed in what he calls dynamization, or potencies.

But it is certain that he held the opinion that the latent properties of drugs were developed independently of the diluted material substance, for he insists that the name of dilution should no longer be applied to potencies where that latent power is made to act in a spiritlike manner upon life; that is, upon the sensitive irritable fibre. While Hahnemann's idea of the subject does

not absolutely preclude the progressive divisibility of substance, based on the possible infinite divisibility of matter, yet those who came after him abandoned this opinion, and unequivocally substituted a spiritual agency in the place of simple material attenuation. As far back as vol. x of the *Algem. Hom. Zeitung*,* "dynamopathy" and "Hahnemannism" were proposed as substitutes for the name homœopathy. The anonymous author was probably Dr. Hering, for he sought to substitute a newly awakened agent, a new fundamental force, which he supposed existed between the original substance and the vehicle. This he called "Hahnemannism," from admiration of Hahnemann, who still hesitated to express this opinion unequivocally in print. Hering's idea was based on the assumption that atoms of one being could impart their character to those of another; and this power was supposed to take an intermediate position between galvanism and mesmerism. Glass and cork were supposed to isolate it quite as well as they do electricity, etc.†

Such ideas as these harmonized with the previous inventions of Korsakoff, and were founded upon them; and it was from these same inventions that arose the process of contact-potencies.

According to this theory a hogshead of water could be made to possess medicinal virtues by contact with a pellet of the 30th. Hahnemann, however, had declared such medication to be impossible, because such a mass could not be stirred or shaken.

My object here is not to give a history of the theory of potentiation, but merely to show its origin and import. The basis of material attenuation was given up, and the theory of high potencies was based on the assumption of the spiritual or of some disembodied agency. While in Germany less and less was heard of them after the great revolution of 1848, in this country an enthusiastic party arose in their favor, awakening the controversy anew.

In order, then, to have a firm basis for potentiation, and particularly for high potencies, the upholders of such a dogma must depend upon a belief in a spiritlike power. And to develop

* Kleinert, History of Homœopathy.

† Kleinert, History of Homœopathy, from Archiv, vol. 15, No. 1.

such a power would require at least some machine adapted to the force of potentizing machines, to which it has been subjected lately.

Arthur Lutze, in Koethen, claimed a much shorter method than this; he simply imbued his doses with animal magnetism. He acquired an immense practice, accumulated wealth, and edited a new edition of the *Organon*, which he enriched with contributions setting forth his discovery.

These doctrines are accepted by some of the followers of Hahnemann, but the majority reject them, except so far as the presence of matter is admitted. No one, except potency-makers, ever attempted to determine where the lower potencies end and where the higher begin; such a distinction is perfectly arbitrary.

Among those who incline, by belief or experience, to the so-called high potencies, there are many who never sought to account for their stability. They never knew or cared to know that they were based openly and avowedly on spiritual belief, and upon the open disavowal of material influence; it was with many faith in the pretentious name of "high potency."

There are many, on the other hand, among those inclined to the so-called low potencies who can state no distinct reason why they prefer them, except by experience and belief. Those who cared to have reasons for their actions surmised, or knew that there was a limit somewhere to the divisibility of matter, and while that limit was uncertain they preferred to remain on the side of reasonable safety, within the domain of the material effects of substances.

Such are the well-known differences of opinion—for they were neither more nor less—from Hahnemann's time up to the present. I have sketched them briefly without voluminous quotations to support them. To do so would be like proving by quotations that the sun rises and sets. The differences of opinion exist; upon them are based different parties of our school, and I believe that I have not done them injustice by overstatement.

In view of these facts, and in view of the acknowledged abandonment of the material principle, and the avowed acceptance of a spiritlike principle capable of being potentized and propagated in the potencies in contradistinction to dilutions, it was unneeces-

sary to adduce, as I have done, the evidence in illustration of the grained and molecular constitution of matter. The spiritual wing of our school will find no hesitation in admitting it, for it neither militates against nor supports their belief.

On the other hand, physicians, of whatever religious views, who, in their dealings with the material world, prefer to remain on the side of material influences, and who would not knowingly push their dilutions to that degree where all material influences must cease, should strive to know where that limit is.

It was for their sake, and also to contribute something towards the knowledge of this limit of divisibility, that I have given a brief sketch of the progress of molecular science. Those able workers in that field labored quite independently of, and without regard to, the bearing which their results might have upon the method of diluting or attenuating medicines.

By again referring to their figures it will be readily seen that matter is all but infinitely divisible. A molecule, though transcendently small to the perception of our ordinary minds, is nevertheless measurable, weighable, and its motions calculable and perceptible to skilled minds.

CONCLUSIONS BASED ON THE PRESUMPTIVE CORRECTNESS OF MOLECULAR SCIENCE.

The conclusions are few and simple. Instead of an infinite scale of attenuations let us have a standard, like weights and measures, which we can teach to learners, that they may not be divided into sects injurious to our cause. Let us declare the 11th centesimal as quite high enough. Let us also abolish the absurd method of endeavoring to dissolve insoluble substances. Still, if any one prefers to continue his triturations and dilutions higher, no one need object. But never let the potency enter into the definition of homœopathist.

To those whose office it is to teach, certain standards of value and measure are indispensable. If we abandon the substratum of matter, we are launched at once into metaphysical speculations, and plunge into spiritual and spiritistic speculations, from which may our school be saved! It will thrive just in proportion to its

fealty to the known principles governing matter. It will fail, as a science, when it seeks support in the spirit world. Objections to these propositions are and will be raised by demanding proofs that higher attenuations than the 11th are incapable of curing. In my last report to this bureau I stated the ground upon which alone the proof of a cure can be established, namely, upon a system of proofs and counter proofs, according to which, in a given case of cure, the result can or cannot be attributed to the medicine.

I have also freely stated that the same applies to the low attenuations and triturations. I can only sketch out my line of argument.

1. Aside from all light which science has of late thrown upon the constitution of matter, "experience" is and always was the only guide in the practice of medicine, in all schools and with all potencies.

2. No decision will be possible in regard to the merits of various potencies until "experience" includes numerous and accurate statistics, obtained from hospitals and private practice. Experience must be based on statistics which show the negative as well as the positive results of treatment. Hitherto only favorable cases have been reported.

3. Until then the assertion of individuals who exclude negative results, and the assertion of superiority of skill and success, is entirely without force.

4. The chances of efficacy of a drug preparation are absolutely on the side of the scale attained before the limit of material presence is reached.

5. Rigid and persevering research and inquiry were the foundation of the new school, and by them only can it be maintained.

These inquiries were undertaken from a desire to lay a safer foundation for the estimation of cures upon the homœopathic rule or law, and thereby to do better than we are all, without distinction, able to do now. Bold and over-enthusiastic assertions of superiority, based on belief in spiritual influences, carry before them the young men of the profession, but are without weight in the light of calm reason.

"*Aude sapere*," said Hahnemann, not "*aude credere*." "Dare to know," not "dare to believe."

ON THE PROOF OF THE PRESENCE OF
MEDICINAL POWER IN ATTENUATIONS
ABOVE THE SIXTH DECIMAL,

FROM THE STANDPOINT OF THE SCIENTIST.

BY LEWIS SHERMAN, M.D., MILWAUKEE.

IN regard to the question, whether solution of the so-called insoluble substances is really accomplished by the Hahnemannian process of attenuation, opinions are divided.

There is abundant reason to believe that no substance is absolutely or entirely insoluble in water and alcohol.

Water and many other substances in the *solid*, as well as in the liquid state, are known to give off vapors at ordinary temperatures. There is a constant tendency of all matter to assume the gaseous state. The higher the temperature the more rapidly does this process go on. The boiling-point of a liquid is by no means the lowest temperature at which it gives off vapor; it is rather that degree of heat (molecular activity) at which the expansive force of the vapor is sufficient to overbalance the pressure at the surface of the liquid. As the pressure diminishes the boiling-point becomes lower. Water will boil at its freezing point if all pressure be removed from its surface. Ice gives off vapor of water with appreciable rapidity when the temperature is many degrees below the freezing-point. Mercury is known to give off copious vapors in the liquid state. Even gold rapidly assumes the gaseous form if the temperature is raised high enough. In the liquid state its vapors are perceptible, and analogy would add to the conclusion that the process is not entirely stopped

when it assumes the solid form. We are by this reasoning led to believe that every substance, solid as well as liquid, is surrounded by an atmosphere of its own vapor. Water and alcohol readily hold vapors in solution. Hence all substances may be regarded as soluble in water and alcohol.

The state of subdivision, produced by trituration, promotes rapid solution by virtue of the extent of surface exposed, and also on account of the fact that fine particles are held in suspension in these liquids for a considerable time.

I do not affirm that all of the medicinal particles of the so-called insoluble substances in their 3d centesimal trituration are dissolved in the 4th attenuation made by Hahnemann's process. I only assert my belief, with reasons for this belief, that some portion of the medicinal substance is dissolved. In most cases these will be coarse particles, which will fall to the bottom of the bottle in the 4th attenuation, and in all cases some particles will remain suspended in the liquid for an indefinite period. Although I believe that the lower Hahnemannian dilutions are solutions of the drugs they purport to contain, I am free to maintain that the process of solution destroys the pathogenetic and therapeutic properties of those drugs. My reasons for holding this last were stated in a paper read before this Society at its last previous meeting. A brief recapitulation may be allowed.

The specific poisonous properties of the insoluble substances depend upon their insolubility. The poisonous effects of lead, for instance are produced by the introduction of the carbonate of that metal; they are removed by administering a substance which renders the lead soluble, such as Iodide of potassium. The Iodine forms a soluble salt with the lead which is excreted by the kidneys. Large quantities of Acetate of lead may be swallowed without danger of lead poisoning, because this salt is soluble and removable by the excretory organs. The specific effects of the different insoluble poisons vary with the physical properties and conditions of those substances.

Metallic mercury in mass has very little effect on the human body. When subdivided into minute globules its great weight and smoothness render it the most active agent in the whole *Materia Medica* for the removal of passive congestions and plas-

tic deposits in the tissues. Metallic mercury, metallic gold, and other insoluble substances in the form of solid particles are known to be taken into the circulation, and deposited in the tissues, causing disorganization or foreign growths. A globule of Mercury may be pictured rolling through an obstructed capillary, like a leaden ball through a long narrow tube, when a ball of some lighter material would only increase the obstruction. The effects of the mechanical action of insoluble particles introduced into the circulation may be as various as the physical properties of the particles. But these effects cannot be produced by the introduction of solutions of those substances, except in cases where the solutions are so concentrated as to form precipitates after they enter the body.

Does trituration promote the solution of gold?

The average diameter of the particles of precipitated gold is about $\frac{1}{300000}$ inch. (The particles of the phosphorus precipitate are somewhat smaller.) The weight of one such particle would be $\frac{1}{700000000000}$ grain.

Hence, there would be seventy such particles in one grain of the 9^x trituration.

A powerful microscope can only make visible at one view $\frac{1}{100000}$ of a grain of a trituration.

Hence, there would be in the 9^x trituration one particle to every 140 fields, provided the particles are not subdivided by the process of trituration. This agrees with the facts of observation.

The free surface in a grain of precipitated gold is about 245 square inches.

If the process of trituration subdivides the particles so that the free surface of gold in a grain of the 9^x trituration is at least as great as that in one grain of the crude, the average diameter of the particles would be $\frac{1}{3000000}$ inch.

The weight of one of these particles would be
700000000000000000000000000000000 grain.

The smallest visible particle with the best microscope and the best illumination is $\frac{1}{64000}$ inch in diameter.

A spherical particle of gold of this diameter weighs about $\frac{1}{9000000}$ grain.

ON THE PROOFS OF THE PRESENCE OF
MEDICINAL POWER IN ATTENUATIONS
ABOVE THE SIXTH DECIMAL,
FROM THE STANDPOINT OF THE THERAPEUTIST.

BY LEWIS SHERMAN, M.D., MILWAUKEE.

UNDER this head I desire to say a few words in favor of the *test proposed by the Milwaukee Academy of Medicine*.

The proposition is briefly stated thus :

A vial of pure sugar pellets, moistened with the 30th Hahnemannian dilution of Aconite, and nine similar vials, moistened with pure alcohol, so as to make them resemble the test pellets, shall be given to the prover. The vials are to be numbered 1, 2, 3, 4, 5, 6, 7, 8, 9, and 10. The number given to the Aconite vial shall be unknown to the prover, and it shall be his task to determine which of the ten vials contains Aconite.

These preparations are to be put up with the greatest care, in the presence of the members of the *Milwaukee Academy of Medicine*, and then placed in the hands of an unprejudiced layman of unimpeachable honor, who shall number and dispense the vials as they are called for by the provers.

The provers must be physicians of acknowledged ability, who possess a good knowledge of the recorded symptomatology of Aconite, and who have faith in the efficacy of the 30th dilution.

If a hundred physicians engage in making the test and all, or nearly all, single out the Aconite pellets, the inference will be that the 30th dilution represents the medicinal properties of Aconite.

If only about ten out of the hundred succeed in the trial, the inference will be that the 30th dilution of Aconite possesses no medicinal properties, for, according to the laws of probabilities, about one in ten would guess right without making any trial.

Preparations of *Arsenicum album*, *Aurum metallicum*, *Carbo vegetabilis*, *Natrum muriaticum* and *Sulphur* in the 30th Hahnemannian dilution, made with the same precautions and care as this of *Aconitum*, shall be used as a

test of the *therapeutic* powers of the 30th dilutions. In consideration of the inconvenience of experimenting on the sick, arising from popular prejudices, the number of vials of "unmedicated" pellets may be limited to one for each remedy, and the experiments tried mostly in chronic diseases. The real gain to the healing art, which will be accomplished by the establishment of the truth or falsity of the theory of "potentization," will amply compensate for the risk of delaying a few cures.

The experimenters must be physicians of acknowledged ability, who possess a good knowledge of the therapeutic indications of the remedies tried, and who profess faith in the efficacy of the 30th dilution. If in this trial there be about one hundred per cent. of successes, the inference will be that the 30th dilutions have curative powers. If there be only about fifty per cent. of successes, the inference will be that the 30th dilutions have no curative powers.

If those who advocate the use of these preparations refuse to participate in the experiment, the profession will have reason to suspect that they are insincere.

If the result of the test should be to prove that the 30th dilution of a drug can make the sick well or the well sick, then it must be acknowledged that in this a great discovery has been made in physics as well as in medicine, and the science and ingenuity of the civilized world will be set at work to find out the useful applications of the discovery.

If the result should be to prove that the 30th dilution has no such powers as it is claimed to have, then the medical profession has a right to demand that the symptoms supposed to have been produced by the 30th and higher dilutions be expunged from our *Materia Medica*, and that advocates of the potentization theory shall henceforth cease to prate their "cures" in medical journals and before medical societies which are avowedly devoted to the interests of science.

Report of a Committee appointed by the Milwaukee Academy of Medicine, for the Purpose of making Arrangements to Prepare a Scientific Test of the Efficacy of the 30th Hahnemannian Dilutions.

MR. PRESIDENT: Your committee have carefully considered the plan proposed in Dr. Lewis Sherman's paper for testing the efficacy of the 30th Hahnemannian dilution, and we are unanimously of the opinion that the test proposed in that paper is fair and honorable, and that the interests of science demand that it should be made.

We recommend,

That our society undertake to carry out the provisions of this test, and that to this end the essential features and the practical details of the test be given for publication as soon as practicable to every regular homœopathic periodical printed in the English language; and that translations of the same be sent to every known regular homœopathic periodical printed in foreign languages; and that all other appropriate and accessible means be employed to give the test publicity.

That the directions given by Hahnemann for the preparation of the 30th

dilution be followed with the most scrupulous exactness; that the alcohol used be of the purest quality obtainable, and that to this end a quantity of the best so-called "homœopathic alcohol" be redistilled in glass for the purposes of this test.

That the Rev. George T. Ladd, of Milwaukee, be selected to number and dispense the vials of test-pellets as they are called for by the provers and experimenters; and that he give a solemn pledge that he will not, in any manner, reveal to any person which of the preparations coming from his hands have been medicated with the 30th dilution, until he shall have been called upon to do so by this society, and that he will use every means in his power to preserve the purity of the materials intrusted to his care, and to make the test fair and honorable.

That all provers and experimenters be required to send their reports to the secretary, Dr. Albert Schlœmilch, before the first day of December, 1879; and that the result be published in full about the first of January, 1880.

And finally, that this society appropriate a sufficient sum of money to defray the expenses of furnishing and delivering the test-pellets of Aeonite to one hundred provers—these being selected from the first who apply—and that the other provers and experimenters be required to pay in advance to the secretary of the society the sum of thirty cents for each set of test-pellets sent them.

EUGENE F. STORKE, M.D.,
ROBERT MARTIN, M.D.,
E. M. ROSENKRANS, M.D.,
JULIA FORD, M.D.,
ALBERT SCHLÆMILCH, M.D.,
G. C. McDERMOTT, M.D.,
O. W. CARLSON, M.D.

MILWAUKEE, December 3d, 1878.

At a regular meeting of the Milwaukee Academy of Medicine, held April 1st, 1879, the following resolution was unanimously adopted:

Upon application by any professor in a medical college, or any other public advocate of the high potencies, the Academy will prepare and furnish the 30th Hahnemannian dilution of *any remedy* in common use, for the purpose, and in accordance with the terms heretofore published in the pamphlet, entitled *A Test of the Thirtieth Dilution*.

A. SCHLÆMILCH, M.D.,
Secretary.

April 21st, 1879.

The most important feature of this test is the removal of all bias from the minds of the experimenters. Whatever preponderance there may be in the selection of medicated pellets, it can-

not be ascribed to the vanity, pride, prejudice, superstition, or love of gain, or any other selfish motive acting on the physicians who undertake the experiment. Hence, if the results should be favorable to the efficacy of the high dilutions, the evidence is incomparably superior to that obtained from men who are of necessity subjected to outside influence.

Men who desire to learn the truth in regard to the question of attenuation and potency will be ready to undertake this test ; but men who have selfish motives for making it appear that the truth is on one side or the other of this question, will neither undertake the experiment nor be willing that others should do so.

Another noticeable feature of the test is that the medicinal preparations are made especially for the purpose of the test by a body of men representing *both sides* of the question, and then immediately put into the hands of a disinterested party. There is, therefore, no chance for collusion or for insinuations of trickery.

The method of preparation is that followed by Hahnemann, and by the most careful pharmacutists of the present day. Thirty vials are used in the preparation of each remedy, and ten shakes are given to each vial. None of the shoddy methods of adventurous heretics are countenanced. The preparations are made with strictness as to method, and great care in regard to purity.

Another important feature is that the experiment is performed by *believers* in the potencies, who are, of course, most interested in the success of the demonstration. In making up the results none but men of acknowledged skill and ability will be counted. Ample time is given for every believer in the potencies to secure the aid of every sensitive subject of his acquaintance. No failures then can be ascribed to lack of skill or opportunity. Each experimenter, moreover, is allowed to choose the particular remedies with which he is best acquainted, and from which he has seen the most marked results in the 30th dilution.

The remedies preferred by the Academy are *Aconitum*, *Arsenicum*, *Aurum*, *Carbo vegetabilis*, *Natrum muriaticum*, *Sulphur*. We think that this list includes the best known and the most active remedies in the Materia Medica, and gives an assortment of those best adapted to both acute and chronic cases, of those which

are poisonous, and those which are non-poisonous in the crude state and lower attenuations, and of those which are soluble and those which are insoluble. If any believer of acknowledged skill and ability thinks he can do better with some other medicine, the Academy are ready to furnish that medicine.

In the trials on the sick there is one vial of medicine and one of blank pills, and the experiments are recommended to be tried mostly in chronic cases.

On the ground of morality we believe this experiment is not only entirely justifiable, but actually demanded. If there is truth in the doctrine of potencies it is important that the medical profession and the world at large should know it, for many valuable lives are lost by the unnecessary use of large doses of medicine. If there is no truth in the doctrine it is important that the medical profession and the world at large should know it, for many valuable lives are lost by the neglect of this great discovery.

APPENDIX.

DISCUSSION.

THIRD DAY—MORNING SESSION.

Answers to the Questions on "Drug Attenuation in Homœopathic Therapeutics," by Ad. Lippe, M.D., Philadelphia.

Question 1. History of Drug Attenuation in Homœopathic Practice, up to the Death of Hahnemann; with a Statement of its Objects and Methods.

Answer. A true and fully reliable history can only be made up from facts which cannot possibly be contradicted, and such facts can only be established through *documentary evidence*. Hearsay testimony is *no evidence*; documentary evidence cannot be contradicted. We find Hahnemann an advocate of the 30th potency in all his writings. The first remedy, by him, published in his *Materia Medica Pura*, third edition, 1830, was Belladonna; he distinctly recommends, as a suitable and sufficient dose to cure, the smallest part of a drop of the 30th potency. Hahnemann says, on page 12 (Preface to Belladonna): "Taught by hundreds of experiments on the sick, I found myself compelled, during the last eight or ten years, to come down to the decillionth (30) potency, and I find the smallest part of a drop (the one thousandth part of a drop) just sufficient to accomplish the healing object." Hahnemann's objects and methods were, first to so diminish the quantity and attenuate the drug till the curative object was obtained without the painful and even injurious effects of large doses of the drug, homœopathically indicated, when administered in such doses as will frequently cause much unnecessary sufferings, and even cause injuries, because given in too large a dose. His method he has frequently described, and did so again in his preface to Belladonna. He there repeats what he had described before as his mode of attenuating crude drugs for better clinical uses. He says, there, that he "takes as a unit

the juice of the plant mixed in equal parts with two drops of alcohol, and drops this into a vial filled with 99 drops of alcohol, shakes this vial downwards twice, which constitutes the hundredth part of a dilution; of it another drop is mixed with 99 drops of alcohol in a clean new vial, and is shaken in like manner, which constitutes the 10,000th dilution, and another drop of it is again shaken with 99 drops of alcohol, which constitutes the 1,000,000th dilution. The potentization is so continued through 30 vials, and thereby we obtain the decillionth potency; with this preparation the homœopathist cures all such cases as are curable with *Belladonna*. He further says, that this small dose is all-sufficient in the most acute diseases, and when given in a chronic disease this very small dose will act for three weeks and longer.

Question 2. History of Drug Attenuation in Homœopathic Practice, since the time of Hahnemann; with a Statement of its Objects and Methods, with Especial Reference to Variations from those Approved by Hahnemann.

Answer. The first known drug attenuations differing from Hahnemann's method were made by Korsakoff during Hahnemann's lifetime, without his objections to this new method, which consisted principally in making "contact potencies;" that is, after reaching, say the 30th potency of Hahnemann's, he mixed a large number of unmedicated pellets with a few medicated dry pellets, shook them very thoroughly, and in this manner he continued to make his (contact) higher potencies. Next came Jehnichen, who produced the 200th and higher potencies; he even produced the 40,000th of Arsenicum. All we know positively of his *modus operandi* is, that he used much larger potency vials, a larger fluid vehicle, and a larger force by his powerful arm than Hahnemann did.

In 1867 the American Institute of Homœopathy, finding that the preparation of drug attenuation was carried on to a much higher degree than had been done by Hahnemann, instituted some inquiries, and addressed itself to Dr. B. Fincke, who evaded a positive answer. This statement of facts was published and the question was agitated in the third volume of the *Hahnemannian Monthly*, page 214. This brought a reply from an eminent lawyer in Brooklyn, and in the same volume of the *Hahnemannian Monthly*, page 385, I was requested to shed some light on the mooted question; this was done in various papers, to be found in the same volumes of that journal, pages 385, 419, 497. The student of the history of drug attenuation will there find "*documentary evidence*" to show who was, and still is, the custodian of the statements made to him by Jehnichen respecting the preparations of his high potencies; it will also be seen that said cus-

todian of the secret was authorized by Jehnichen to divulge and promulgate it "*when the right time came.*" Like Jehnichen, Dr. B. Fincke, too, who produced fluctuation-potencies and much higher drug attenuations than any one before, kept his secret. In 1859 the late Dr. von Boëninghausen caused Lehrmann, who was a trusted pharmacist, to prepare the 200th potency of 200 medicines after Hahnemann's method. These various preparations were well tested by experts and their testimony laid before the profession. The fundamental law of demand and supply became evidently the cause of a multiplying preparation of high potencies. There was such a demand for them that we find the late Dr. Carroll Dunham, by the aid of Dr. H. M. Smith, in New York, prepared another set of the 200th potency, much more shaken than were those of Lehrmann, who gave each vial only two downward shakes, but used for each potency a new vial and alcohol for a vehicle. Then came the preparations of Boericke & Tafel, of Dr. Samuel Swan, and of Dr. Thomas Skinner. There was no secrecy about these latter preparations; the methods used were published in our journals.

The object the various drug attenuators had in view was undoubtedly a desire to find a potency which would no longer show the presence of medicinal virtue, would be administered to the sick without causing any curative or other effect. The experiments made with Jehnichen's high potencies showed that the higher potencies possessed proportionately greater curative powers than the lower potencies, and when drug attenuation was carried still farther, it was only to prove the correctness of the former observations.

Question 3. The Means Employed in Drug Attenuation; what they Should Be, and the Danger of Impurity.

Answer. The question has really been answered before. What they should be? Why, they should be so prepared as to produce the most efficacious medicines, which they cannot do if impurities are allowed to become part of the preparation. Nothing but the experiment can decide whether an attenuated drug is pure and the attenuation useful in curing such cases of sickness as fully correspond with its sphere of action.

Question 4. The Limits of Drug Attenuation, or Proof of Drug Presence in Attenuations above the Third Decimal; from the Standpoint of the Scientist.

Answer. No doubt the question truly means, from the standpoint of the materialist. The scientist stands for "materialistic investigator." Whether any particles of a drug can be detected in a preparation above the 3d decimal is by no means settling the grave question as to the development of the curative powers

of drugs by potentization. The microscope has detected particles of triturated metals far above the 3d decimal, and there are now investigations in progress showing that by the solar spectroscope much higher triturations show discernible color lines. Such questions (as No. 4) have been asked by the opponents to homœopathy time and again, and we hope that some of our *scientists* will make it a point to show how far these investigations have gone; and how far they show the presence of drug particles in drug attenuations will show how far materialism finds itself eliminated by the spectral analysis. And we may just here be permitted to call attention to a very becoming answer to this question, which we find in the *Hahnemannian Monthly*, page 198, by S. A. Jones, M.D. It reads thus: "Now why may not a homœopathic therapist, as well as an old-school physiologist, apply the *physiological test* to determine the presence of a poison in quantity too minute for chemical test? And why oblige the homœopathic therapist, when he has successfully applied the physiological test, to demonstrate the condition of the 'original substance' in the thing tested; why insist that he must show whether the 'original substance' be in minute subdivision or in solution? Why deny the existence of some of the 'original substance' when, though the microscopic test and the spectroscopic test fail, the physiological test gives affirmative evidence? Is the marvellous human body clumsier than the microscope, less sensitive than the spectroscope?" Professor S. A. Jones, in his laudable efforts to enlighten Professor Conrad Wesselheft, who had unfortunately fallen into a *Carbo veg.* demonstration, might as well have told him that the provings of *Carbo veg.* were made with the 3d and higher triturations, the crude substance and the first two triturations having produced no effect on the provers.

Question 5. The Limits of Drug Attenuation, or Proofs of the Presence of Medicinal Power in Attenuations above the 6th Decimal, from the Standpoint of the Therapist.

Answer. The limits of drug attenuation are *unknown!* There has not yet been produced a single drug attenuation which, subjected to the test of the therapist, has not shown strong, nay, so far, steadily increasing medicinal and curative powers. Proofs of the presence of medicinal power in attenuations above the 6th decimal are in the possession of the medical profession by the thousands; they are on record in Hahnemann's *Materia Medica Pura*, in the medical journals published in the last *fifty* years. It is, therefore, a settled question, settled forever, and the very admission of the presence of medicinal power in the 6th decimal attenuation is implicitly an acknowledgment of the cor-

rectness and admissibility of the theory of potentization ; or does there exist anywhere a "nothing?" Can any substance come to nothing in any way ; or, by division, be divided and finally come to be *nothing*?

The idea of nothing is absolutely the negation of all ideas ; so is the negation of the existence of the substance, in an ever-so-much divided substance, a negation of the existence of the substance itself before its division.

The presence of medicinal power of any undivided or crude substance can only be ascertained by the clinical experiment ; in like manner can the presence of medicinal power in any attenuation only be ascertained by the clinical experiment.

The testimony, so abundantly offered, positively *affirming* the curative power of highly attenuated medicines, can never be set aside save by a more abundant testimony showing exactly the contrary. But, the admissibility of such testimony would very much depend on the qualifications of the witnesses. If a witness testifies that in his hands potencies above the 6th decimal have not shown any medicinal (curative) powers, he will have to show that he carefully selected his remedy and that he skilfully applied it. Till that is done fairly and honestly, the testimony as we find it recorded for more than fifty years will stand unshaken, and be final.

MEANS EMPLOYED IN DRUG ATTENUATION.

BY M. J. RHEES, M.D., WHEELING, W. VA.

SOME facts in regard to drug attenuation, and especially to that important part which is accomplished by trituration, have forced themselves so strongly on my attention that I desire to lay them before you.

Accompanying this paper you will find a package of powders, each bearing a number, for my own convenience of reference, and each, except the last two (Nos. 7 and 8), claiming to be the 2d decimal trituration of Kali bichromicum.

Nearly four years ago, having a case of malignant diphtheria under treatment, I sent for one of the most eminent homœopathic physicians of Boston, a professor in the Medical Department of Boston University, to meet me in consultation. (I was then living near Boston.) He advised Kali bich. 2^x, and Hepar s. c. 2^x, to be given alternately. As I had very seldom had occasion to use those preparations, I had none on hand, and the doctor promised to send me some as soon as he returned to Boston. He did so, and powder No. 1 is a specimen of the Kali bich. received from him, poured out of the very vial in which it came to me. When I looked at it, I was astonished that any physician could be willing to accept it or use it as a trituration of any degree; but thinking it might have been prepared and sent by one of the doctor's students without his supervision, I gave very little more thought to it.

Last December I removed to this city, and have since come into possession of a two-ounce vial of stuff marked Kali bich. 2^x,

which I know had been extensively used by a physician of some note in the West, who was formerly a professor in a Western homœopathic college. Powder No. 2 is a specimen of this preparation.

In the course of a professional life of a third of a century, I had made too many triturations, myself, not to be able to see at a glance that these were in no sense *triturations*. I therefore determined to find out if possible, whether any reputable homœopathic pharmacy sent out such mixtures as triturations, and knowing with which of the pharmacies the last-mentioned physician usually dealt, I wrote to that one (a prominent Western pharmacy), and also to the most prominent pharmacies in the Eastern and Middle States, ordering from each, one drachm of the 2d decimal trituration of Kali bich. Powders 3, 4, and 5 are specimens of what I received from each. Nos. 6, 7, and 8 are triturations made by myself.

It will be perceived that even in No. 5, which is the best obtained from any of the pharmacies, great "chunks" (to use the expression of one of our eminent microscopists) of the crude drug are clearly visible to the unaided eye. Larger and more numerous "chunks" are seen in each of the others.

It is not possible that any of these preparations were made according to Hahnemann's rules, or with any desire to accomplish the true purpose of trituration. There are two objects to be attained in the trituration of homœopathic medicines. The first is, complete subdivision of the drug or crude substance and its intimate mechanical admixture with the vehicle; the second is, DYNAMIZATION. The second and most important object cannot be attained unless the first is accomplished. But what constitutes complete subdivision of the drug? Can the subdivision reached in five of these specimens be called complete or even approximately so? I think not. They each, from the crudest mixture to the best machine-made trituration, contain masses of particles large enough to be seen without a magnifying glass. Perhaps we cannot reach absolutely complete subdivision down to the ultimate molecules; but we can certainly come nearer than have these pharmacists. I think there ought to be no particle of the crude substance large enough to be seen by the microscope in the

2d decimal or 1st centesimal triturations, if it has been properly comminuted. If the 1st trituration has been properly performed and sufficiently prolonged, the molecules of the drug ought to be separated and equally distributed among the molecules of sugar of milk, and even if the ultimate molecules cannot be entirely separated, the mass can be, and ought to be, proportionately and equally divided. I think there may be in each of these specimens of triturations I send you, some separate molecules of Kali bich.; but there are also, in at least five of them, masses containing millions and billions of molecules in their crystallized agglomeration intact. Having escaped subdivision in the 1st decimal trituration, these masses have also passed through the 2^x. If any mass of molecules passes the 1st trituration, whether that be a decimal or centesimal trituration, without subdivision, its chances for escaping in the second and all subsequent triturations are multiplied. For this reason I am not surprised that Professor S. A. Jones found gold visible under the microscope in the 9^x. He found a particle or particles, or more properly, masses of molecules which had accidentally escaped comminution in all previous triturations, through imperfection in the manipulation, and he may happen to find them in the 30^x from the same cause. But if the 1st and 2d triturations had been properly made, he could not have found visible particles in the 9^x, if even in the 3^x or 4.

If in the case of any drug, the 1st trituration is not carefully and properly made, and the exact proportion of molecules preserved throughout every part of the trituration, it is self-evident that no subsequent trituration or attenuation can be correct; therefore, it is of the utmost importance that the 1st trituration of every drug be made with the most perfect exactitude. The pharmacists may say, and truly, that in the large quantities in which they prepare these triturations, it is impossible to make so complete a comminution and perfect mechanical admixture, as when they are made in smaller quantities. But if they cannot be made properly in large quantities, and by machines, they ought to be made in small quantities, and by hand. This is work that requires brains. Let us by all means have true and scientifically correct preparations of our medicines, whatever it

costs. Whether the molecular theory is true, or matter is infinitely divisible ; whether our medicines are dynamized, and their medicinal power is communicated to the vehicle, or not ; whether we give the low triturations, or the 30th centesimal, or the 200th centesimal, let us have our 1st trituration and 1st dilution made as if upon *them* human life and all its interests depended.

Four facts in regard to trituration have been considered in the preceding pages. The first is : That some physicians, claiming to be homœopathists, and holding positions of honor and responsibility in the profession, even in classical Boston, with her supercilious assumption of scientific supremacy, as well as in the somewhat less pretentious West, are content to use preparations in the treatment of disease, which are in no sense homœopathic triturations, but are absurdly crude mixtures. The second is that our oldest and most reliable pharmacies dispense preparations under the name of triturations, which, though better than the crude mixtures considered above, are very far from being perfect, even in the case of Kali bichr., which is much more easily manipulated than many others. The third fact, as shown by the samples I send you, is that, triturations dispensed by the different pharmacies, but bearing the same name and number, do not agree in color ; thereby showing a difference in proportion of crude drug to sugar of milk. The fourth fact, as proved to you by my own preparations, is that far better triturations can be made without difficulty by means of a common mortar and pestle, than are made by the machines of the pharmacies. Whether the machines, when under the control of conscience and brains *can* make thorough triturations, or not, I do not know ; but I do know that we have the proof before us that they *do not* make them in all cases.

To preserve the purity and identity of our remedies a separate mortar, pestle, and spatula should be used for each drug. In some cases the horn or ivory spatula will be indispensable ; but it is a question worth considering whether, in most instances, an extremely hard and sharp steel spatula would not be better. It would be least likely to lose any portion of its substance in scraping the smooth surface of mortar or pestle, and is, in my opinion, the best instrument for the purpose, except when a chemical sub-

stance which could attack and combine with the steel, is being triturated. The mortars and pestles should be thoroughly washed and scoured with Bristol brick or white sand, and washed again in hot water and dried by heat after every trituration. As, often, the atmosphere surrounding the operator becomes charged with impalpable and invisible particles of the medicine, every drug ought to be triturated in a small room devoted to this purpose. The room should have a hard floor and no carpets or hangings of any kind about it. It should be heated by steam, have its own water supply, and be kept scrupulously clean. One of the greatest difficulties in making triturations is the great liability of dust and impurities floating in the air, to get into the mortar and become mixed with the trituration. To guard against this the room should be small and otherwise as above described, and the house should be in the country surrounded by meadows or lawns, where a pure atmosphere is possible. The impossibility of making a pure trituration in such a city as this or Pittsburgh is very evident to any one who has ever tried it; it is bound to be contaminated with carbon in the shape of lampblack. But *any* city should be avoided. The proprietor of such an establishment should be a well-bred and experienced physician, of mechanical tastes, and he ought to make every 1st trituration with his own hands. He ought also to have the whole establishment under his own direct supervision. With such a factory of triturations in competent hands we might hope for exactness and purity in the whole scale of our remedies.

Allow me to call attention to the strange indifference of some of our most eminent men to the mode of preparation of the remedies they use. On one occasion, when discussing the high potencies with one of the most distinguished homœopathic physicians in the United States, and trying to show him the ridiculous absurdity of their mode of preparation (that is of Swan's thousandth and millionth potencies), he said to me, "My dear sir, I don't care what they are, or how they are made, so long as they do what I want them to. I cure my patients with them, and that is enough for me." But ought this to be enough for a scientific man? Is it not incumbent upon us all to know with what we are risking the lives of our patients? If we believe that

the 40,000th of *Lycopodium* will cure a certain case of diphtheria, and that nothing but the 40,000th will do it, as some of these gentlemen do, are we, under any circumstances, excusable in giving a preparation, which (as I have conclusively shown elsewhere) may be the 10th, or may be the 3d centesimal, and may be nothing but water, but can by no possibility be what it claims to be? How can these men quiet their consciences in such indifference? and how can we hope ever to have conscientious and exact pharmacists while the profession is itself apathetic and unconcerned as to the quality of the means made use of for the cure of suffering humanity?

DR. T. F. ALLEN: I believe that I am not a fanatic on high potencies, as I have long been known as a low potency man. I really was never above the 200th, though I have occasionally given the 1000th. As a scientific investigator, which I try to be, I would like to see settled the question regarding the efficacy of the 30th potency.

I have settled it in my mind long ago, and would like to see it settled by this body; the only way is, not to write a long article in a great many words, but to set to work and make experiments, and see if we cannot get at the truth of the matter in some way. We may read papers for a week and not be any nearer the truth than we were before. I think the propositions of Dr. Potter and Dr. Sherman are right to the point. They propose to make a test of this matter. I do not consider that their proposition was put exactly in the proper way, but it is a step in the right direction, and I believe that is the only line on which this can be settled properly.

Now, those gentlemen who wish to give us some help can do so by giving us some vials, and letting us fill each of them with the 30th dilution, each of which dilution shall receive the 200th succussion. These half dozen remedies shall be put in the hands of some unbiassed member of this Institute, an honorable, upright man, and a vial of this shall be given to some person who is willing to undertake this trial; and I will say, gentlemen, that I would be perfectly willing to do it. You might give me one bottle of the 30th dilution of any of the six remedies, and I would pick out the right one. I would like to have the remedies run close together in their pathogeneses. Now you will have converted me if I cannot tell the name of the remedy, but if I tell you the name of the remedy, then you must, Messrs. Sherman and Potter, acknowledge the efficacy of the 30th dilution.

Now that is fair. Well, having acknowledged that, next year, gentlemen, I will take you one step further, and compel you to acknowledge the superiority of the 30th dilution over the tincture of the 1st. (Great applause, and cries of Hear! Hear! Hear!)

DR. MORSE: I desire to ask one question of our President in regard to his paper. He has given us the ultimate divisibility of matter into molecules and atoms. I wish to ask, if he has got one atom of that matter, and if he puts that into water, whether it will not impregnate that water indefinitely? I want to know if we have any means of showing that it will not?

DR. CLARK, of New Bedford: I was very much pleased to hear Dr. Allen's statement. It is a very manly statement from his standpoint it strikes me. I am very glad the issue is taken in this way. I think it is high time, for the credit of ourselves as a scientific body, that we cease from speculations in regard to these matters and wordy talk, and commence upon some positive action by which we can govern ourselves, and decide these difficult questions. One thing more I would like to hear from Dr. Allen, and that is, what he got up for? He asks that when this report has been received, if he should determine the medicine which he is trying, that we shall acknowledge that the 30th is efficacious; and furthermore, he proposes another thing which I hope he may live to accomplish. Now I want to hear whether Dr. Allen, a year hence, proposes to acknowledge, in case he does not tell the remedy, that the 30th is not efficacious? (Applause.)

DR. LILIENTHAL, New York: There is said to be a little old book, which I suppose some of you have heard of; it is called the *New Testament*; it is not so new after all. It is an old book, but in that old book there is a chapter and a verse where it says there is a corporal body and a spiritual body, but it says a body in both; and thus I think our medicines are of that kind of spiritual body, and I am not ashamed to own it. I am not afraid to state that the microscope may not find the medicinal gold particle; still, in that water *there is the gold*. We have accounts that the old Indian physicians were far ahead of us in the knowledge of the use of remedies. They called them spirits, and they gave them the right name without knowing why.

Now, I like these tests proposed by Dr. Potter, and I would like to see a test applied. But these tests don't amount to very much to me, and the reason of it is that when men like the late Dr. William E. Payne, of Bath, Maine, or like Dr. Carroll Dunham, made an assertion, men who never left anything undone in their lives, I believed it. Dr. Dunham knew his pathology as

well as any one. When he said it was pneumonia, it *was* pneumonia and not moonshine; and when he says he stopped a disease, that has its usual course of from eleven to thirteen days, with Phosphorus 200th, and the disease passed off, and the patient got entirely well in two or three days, I think it was pneumonia and that Phosphorus cured it. Now, Dr. Potter and the other gentlemen from Milwaukee will find nothing in it; but if a thousand persons should come and say that there was nothing in it I would not believe them. Their assertion would prove nothing to me as to the action of medicine in disease. We have the testimony of trustworthy men who have seen the benefit of it, and I do not want to have better testimony. I do not speak for myself; I ask physicians of high standing who think everything of pathology. If such men say we have done well, I don't see why we should give it up because a scientific body happens to decide against us. There may be a great deal of nonsense as well as a great deal of sense, but it does not answer the question, and it will never answer it.

DR. PEARSON, of Washington: I had resolved to take no part in this discussion; but it occurs to me as I sit here just now that there is a heretofore,—that these papers would appear some time in the far future, perhaps in the *Proceedings*, and I want to say something in opposition to some of the sentiments contained in them. I believe that it is the experience to comfort a man who has fallen from grace.

Now, I take it that Dr. Dake and Dr. Breyfogle are backsliders, and the only way I can see to benefit these men is to entirely change them. There is some more hopes of Dr. Sherman, for so far as I know he has never been converted. He and Dr. Breyfogle gave us some good homœopathy yesterday, but unfortunately for them it was all *copied from Hahnemann's Organon!*

There was one remark of Dr. Dake's to which I wish to refer briefly. If I understood him correctly, and I think I did, I heard the same objection made twenty-five years ago by an allopathic physician with whom I had a discussion. It is simply this, a point urged by these men very generally: the fact that patients recover after the administration of a high potency is given as an evidence of recovery as though it was owing to the high potency. Now I think I can make it plain to every member of the Institute that it was put in this way: that if Dr. McManus gives a few doses of the 30th potency to a patient, and that patient recovers, there is no evidence that the recovery was owing to the remedy; but if Dr. Dake gives a few doses of the 1st or the 3d, and if his patient recovers, it is conclusive evi-

dence that the recovery was owing to the administration of the medicines. (Applause.) That is the proposition simplified.

Dr. McMANUS, Baltimore : After the able remarks you have heard while sitting here, I think it is almost a work of supererogation that I should speak at all ; I believe it my duty, however, to say to this Institute, to the cause of homœopathy, and to the honor of Dr. Hahnemann something in reply to my practice in reference to the use of potencies. Forty-two years ago next November there did not live on the face of the world a greater skeptic than I was, for I was what I have often heard called a regular "wagon-horse" practitioner. I had promised to investigate homœopathy ; I bought some books and commenced the investigation of the 30th attenuation to see whether there was medicine in it or moonshine. I did not know which, because I did not believe ; I commenced by using the 30th attenuation of medicine. Of course I had no experience, but I had been investigating. They all talk about scientific investigation here, and I have been investigating myself, because I do not wish to be a humbug, and I never wish to humbug my patients. I have been investigating this thing for a good while, and I am going to investigate it as long as the Good Master will allow me to live. I need not tell you all that I should be a fool in my own behalf if I did not cure with it.

A member of the Institute told me something in regard to myself I never knew before, and in one respect I consider it a compliment. He said :

"I do not believe that in the whole forty-two years of your practice you ever cured a case of any kind of sickness with the 30th attenuation of medicine. I will not say that all your cases died, because you have the reputation of being a successful practitioner!"

I asked him : "Well, how do you account for their recovery?"

He replied : "Oh ! there are a great many patients of physicians who get well without any medicine,—some from the tact of the doctor, and some from their confidence in the doctor, and all that kind of thing."

Now a man who begins with any attenuation, no matter what, must surely cure his patients or they will die ; and they do die thousands of times for want of professional knowledge. It is a kind of impertinent absurdity on the part of any one to stand and tell a man who has been practicing for forty-two years and prescribing for, on an average, from ten to thirty patients a day, to tell him his medicine never did any good. Would you not suppose I ought to turn around and ask this man, "Do you suppose all these thousands, yea, tens of thousands of cases who have put themselves under my care, required nothing but sugar to get well on?" Is there any person who would make such a

preposterous assertion? No! If any of these gentlemen who are skeptical will come to me and say, "Dr. McManns, do you pretend to cure a fever?" I will reply, "Yes, sir." "Can you cure bilious fever?" I will say, "Give me a ease of what you call bilious fever; let me have entire charge of the ease; I will not tell you what medicine I am giving, if any at all. But I will take upon myself the total responsibility of the case and the result. Then, if I cure that case, which you have pronounced bilious fever, if I cure it, and you see it, will you believe me?" "No, I won't," he replies. (Laughter.) Then I will say to him: "If you cannot believe the testimony of your own eyes and senses, who can convince you?"

Now this Institute is going to be disgraced by these conflicting theories. Some men declare that nothing above the 6th attenuation will cure disease. Why do they say so? Because they never use anything higher. So-and-so told me he never cured a case with the 30th attenuation in his life. I asked if he had ever given it. He replied he had not, but understood that others had. I said I had given it with success for the last forty-two years. But who is to be convinced? These discussions as to the efficacy of the high and low potencies are going to ruin the Institute. It is going to harm homœopathy more than all the allopathic books ever did. (Applause.)

I had the pleasure of travelling with a man I never met before; he lives in Brooklyn, and we began talking on this subject. He talked like a sensible man. He told me that he had been practicing medicine twenty-five years, and had never given anything higher than the 200th; he had more success with that than anything else. I have no objection to any one using what he pleases so he does not call me a fool because I do not get up to the 2000th, nor Mr. A. a fool because he don't come down to the 30th. Whose business is it? And since the practice of every man must correspond with his success, his success will soon demonstrate what he practices. His success must be established by his cures; and if he cures, it is no matter with what potency he does it. I regret in my declining time to see in the distance anything arise that is going to be the death of the career of this Institute. Coming men are bound to differ on this subject. Let them go into a room, shut the doors and windows, put out every one, and then compare notes with each other, and not give way to this spirit of villifying a distinguished man and calling him a blackguard, and all that sort of thing. Where will the thing permanently stop? You must stop it, or otherwise you will have to stop your science. (Applause.)

Just as certain as there is a God in heaven, I say it reverently,

if this thing that is going on now continues, you must abandon your Institute of Homœopathy. (Great applause.) No, let us be like a band of brothers; if we differ in any essential, it is a fact that we differ from necessity. It is for A. to do it, and for B. to do it. Now in the name of Dr. Hahnemann let us be in earnest in this matter. In the name of the Institute, in the name of God, be at peace with each other; and if one considers the other wrong, and if he considers himself to be aggrieved, why, let him in the name of charity do to others as he would have others do to him.

DR. COOKE, Chicago: It strikes me, Mr. President, and it struck me yesterday, that the platform of the American Institute of Homœopathy is not precisely the arena in which to ridicule homœopathy. I would not, however, state this in the first place but for the remarks of my venerable friend McManus. I have only to tell a story not very long. I think that my attention was called to the possibility of the 30th doing something by an act of Dr. McManus, and since that I have gone up the seale; and the older I grow, the sealer I get.

My little baby was brought into the room at my house, at the last occasion of the meeting of this Institute in Chicago, with a ringworm three days in progress over the left eye. We were at the table, and the children were bidding us good-night. Dr. McManus would not kiss the baby, because it had a ringworm. He said: "But I will kiss it to-morrow night, when the ringworm shall be cured;" and, after dinner, he asked me for my *Materia Medica*, for the *Symptomen Codex*. I said I had a very excellent copy at my office. "But," he said, "you cannot stay through the night in your house without the *Symptomen Codex*?" Says I: "I cannot, but I do. I will never do it again." He was not satisfied until he came to my office the next day, and studied my *Symptomen Codex*, and I recollect very well I wanted to be in the Institute while I had to get the tools for Dr. McManus to work with. He seemed to be terribly puzzled and vexed because of the time allotted to him; he could not find the remedy that had a particular symptom over the left eye; he could only find it over the right eye, which I thought was all sublime nonsense,—with all due respect. He, however, gave the medicine as a *dernier ressort*; it was Capsicum which had the ringworm over the right eye, and he gave it to my wife, as he would not trust me with it. One little powder of the 30th of Capsicum to give the baby at night; another to be given in the morning if the ringworm had not disappeared. Now I am not sure, but I think my wife shook me in the middle of the night, and said: "Frank, it looks to me as though that ringworm was fading out; I don't

know whether to give that other dose or not." "Let us wait till morning," said I. "No," said she, "I want to follow instructions." I arose and turned up the light. I said: "I guess I do not see well enough." When the baby was brought to the breakfast-table the next morning the ringworm was almost gone.

The remedy was never repeated, and the ringworm was out of the way in twenty-four hours.

DR. J. EDWARDS SMITH, of Cleveland, Ohio: Ladies and Gentlemen: I have a few words to say. If I take a keg of gunpowder and put a red-hot iron into it, there will be an explosion, and it will hit back; so, if I strike a keg of nitroglycerin, there will be an explosion; so with relation to the discussion of matters that are legitimately brought before us. We can do it in such a manner as will go out and appeal to sensible people all over the world; if we cannot do that, let us dry up and go home. Now, my friend Dr. McManus thinks it desirable for me to follow him, and, gentlemen, I am so much his junior and of less experience, and it is a very ungrateful thing; but as a scientist there are some things that I am bound to preach on when I get the chance.

First, let us notice some of the arguments he has given us, and see how far they will agree. It has been my vocation for several years to lecture to students in a medical college, and I have invariably found the first year student, on his first lecture, knew more about the efficacy of highly potentized medicine and high potencies than any other man. He can discount Dr. McManus, discount his practice. Now, as Dr. McManus tells us and as we all know, he has had an honorable and lengthy practice, has been successful, and has been revered by his fellow-men. We all revere him, and hold him in veneration, but you have got to look straight at the facts. Now, I propose a plan here for the success of which I propose to answer with my life; if it fails you may take my head for a football, and my body for the dissecting-table.

I refer you now to Dr. R. V. Pierce, of Buffalo, with this broad square statement, that he has cured ten men to Dr. McManus's one; and he has got the written evidence of it, and the very best class of evidence, senators, lawyers, ministers, governors; men whose statements are bound to be received and admitted; Dr. Pierce has cured ten men to Dr. McManus's one, and, therefore, *ergo*, sugar is the same thing.

DR. PEARSON: He copied the names from the tombstones.

DR. SMITH: I think he will beat you fairly on that, sir. (Laughter and applause.) Let us stop once more for a moment, and then go on.

First. The gentleman (Dr. Allen) when he got up, was willing to try the Milwaukee test. He was willing to try the Milwaukee test, and he said: "If I succeed, and if I at first point out the remedy, then you have got to admit the efficacy of the 30th dilution." There was a good deal of blaze about my friend out there; he got hold of a chair, and began to pound it. He then said more than that: "If I do succeed you have not only got to own the efficacy of the 30th dilution, you have got to do more, you have got to own that they are more efficacious, etc."

DR. MORSE: I would like to ask you a question. Will the test be a perfect one?

DR. SMITH: Now there is only this to say; it will be if he does it; *if he does it*, then there is time; if he does not, then it is not his place to crow. Make him go to war and fight the battle, then come back and do it over.

DR. DE GERSDORFF, of Boston: I am what is called a low dilutionist, but I am a homœopathist, and I revere the name of the great founder of homœopathy as much as any of you can. I am also happy to say that I belong to those who would not wish to have his memory burdened with all the extravagancies of his late followers. He was a reformer; he had to give strict rules, and had to insist upon obedience and close following; and from that proceeded, in the course of time, a certain peculiarity of the whole sect of medicine to which he belonged.

Hahnemann, as has been often said, would not have been opposed, to-day, if he was promulgating his theory as he was then, by the old school, because he was then ahead of his time and the science of his age. He was even ahead of his own knowledge of physiology and pathology. His grasping genius caught at this method, which he hardly could explain—the mysteries of himself. At his time, and shortly before, the great leading men had a peculiar faith in the certain inherent power of medicine. They sought an arcanum without having found one. Hahnemann found an arcanum. He found the secret, not of one remedy, but of all the remedies, by a certain method, namely, by homœopathy. Certain errors adhered to him in later life; he began to see more importance in the dose than before, he thought more of the remedy than of the other side, than of the recovery of the case. He put, in other words, the dynamization in the remedy, while it ought to be put in the recovered vitality of the body. I have, in my obscure and simple way of practicing homœopathy, never got any further than this way, that when we use it according to these symptoms, from a distinct law, we come to reduce the dose, and we can make a very little medicine do. Owing to what? Not owing to the inherent division of

medicine; that is a physical and chemical body, not a physiological body; a physiological body which is able to have a vital reaction is the sick man; if the homœopathicity exists between the two, then only we get a cure. Therefore I have remained satisfied with my cure in the lower dilutions for almost forty years, and here and there have tried a higher, but I don't find it very necessary. I desire, however, to say to you, I would never wish to quarrel with anybody who can cure with the higher; they can do as they please about that, but I stick to homœopathy and the selection of remedies.

THE CHAIR: The time for the special business has arrived, namely, the special order for 11 o'clock. We will now listen to Dr. Alfred C. Pope, of London. Dr. Dake will close this bureau with a few remarks.

DR. DAKE: Mr. President, it is not my purpose to take up much time. In the first place, it will be clearly understood that the course this bureau has taken is with a view to discuss this subject plainly and squarely. There is no sort of quarrel about it. Now it is the very height of absurdity, it is entirely untrue, that this bureau, in any of its papers, has come in here to occasion a quarrel and to break up this Institute, though my worthy senior friend here seems to fear it. That is all nonsense. We are men, and we claim to be men of science. We claim facts as the basis of our belief; facts pertaining to this body—the human body, the material body and material things acting upon this body—we have not yet gone into the spiritual sphere. I wish to state some very plain propositions and to invite particular attention to them. I do so in answer to claims set up here in favor of the exclusive use of the high attenuations or potencies, and not because the clinical proofs of drug presence and power are especially called for by the subject brought before the Institute by our bureau to-day. From remarks made here a person, unacquainted with the history of homœopathy, might be led to suppose that everything had depended, from the beginning, upon this “spiritualizing” process in the preparation of medicines.

My first proposition is this: *That homœopathy was established successfully and firmly in the world by the use of low attenuations.*

Hahnemann, for the first twenty-five years of his practice, used nothing which the gentlemen on the other side would call high potencies. Beginning with the crude preparations of the shops, he gradually came up the scale of attenuations as far as the 30th. He considered the 30th as high; but others have gone beyond him and, to-day, talk as though his mantle could by no possibility cover anybody not given to the advocacy and use of “potencies” never dreamed of by the master. Men who sue-

cessfully treat more patients in a day than some of these accusing high attenuationists do in a week or a month, are called "backsliders." They are lectured, here, for the very kind of practice that gave solid footing to homœopathy. How utterly absurd!

My second proposition is: *That the low attenuations, having been sufficient for the demonstration and establishment of homœopathy, there is no need of leaving such unquestionable means for those which are mysterious and uncertain.*

Why leave solid ground and go up into the clouds? Was Hahnemann's experience for twenty-five years a failure? Look around, over America, and over England, and over the continent of Europe, and see who have been curing the sick with homœopathic remedies, who have been fighting the battles and winning strongholds for the new school. With here and there an exception they have been such men as are here called "backsliders" by one of the speakers; *they have used low attenuations.* (Great applause.) I deny the claim implied, in the face of facts and history, that these high attenuationists have done all the curing.

A MEMBER: Nobody has claimed it.

DR. DAKÉ: I may mention two well-marked and alarming diseases, in the battle with which our system has won its greatest victories and made its grandest marches, and against which the low attenuations have been almost invariably employed, namely, Asiatic cholera and yellow fever. (Great applause.)

But I cannot enlarge. For myself I will say that I began practice with an outfit of the 30th, thinking to begin where Hahnemann left off; but I was not long in finding it necessary to go down the scale. I have used what is called the 200th and 4000th. I have had experience, up in the clouds, as well as some others from whom we have heard to-day. The experience is not all on one side.

But I will close with the remark that the papers of our bureau have been carefully prepared, that they are candid and respectful, and that not a single statement, either of history or fact, has been contradicted upon this floor.

The aim of the bureau has been to bring out the truth and nothing but the truth for the benefit of the Institute and of the entire profession. We will have the truth and the heavens will not fall.

REPORT

OF THE

BUREAU OF MATERIA MEDICA, PHARMACY, AND PROVINGS.

J. P. DAKE, M.D., CHAIRMAN,	Nashville, Tenn.
C. WESSELHÆFT, M.D.,	Boston, Mass.
W. L. BREYFOGLE, M.D.,	Louisville, Ky.
T. F. ALLEN, M.D.,	New York, N. Y.
J. F. COOPER, M.D.,	Allegheny, Pa.
LEWIS SHERMAN, M.D.,	Milwaukee, Wis.
C. H. LAWTON, M.D.,	Wilmington, Del.
H. M. PAYNE, M.D.,	Albany, N. Y.
J. EDWARDS SMITH, M.D.,	Cleveland, Ohio.
A. C. COWPERTHWAIT, M.D.,	Iowa City, Ia.

AT THE

THIRTY-THIRD SESSION

OF THE

American Institute of Homœopathy.

MILWAUKEE, JUNE 16TH, 1880.

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INTRODUCTORY REPORTS.

BY J. P. DAKE, M.D., CHAIRMAN, NASHVILLE, TENN.

GENERAL INTRODUCTORY.

It is one of the duties of a chairman to present a *résumé* of the discoveries and improvements in the department of medicine committed to his bureau. My excuse for the omission of such service, on this occasion, is that, so little time is given for the presentation and discussion of the special subject selected, we cannot afford to introduce other topics, however interesting.

I must remark, however, in this connection, and for the benefit of my successors, that authors of books and pamphlets, relating to the interests committed to this bureau, and introducers of new articles of *materia medica*, or improved appliances in pharmacy, should send copies and specimens to the chairman, for his examination and notice. In no other way can he be expected to make mention of them properly in his annual report.

Only two books on *materia medica*, out of several, published during the last two years have been thus sent to me, namely, that of Dr. H. C. Jessen, on *Therapeutic Materia Medica*, and that of Dr. Carl Heinigke, translated into English by Dr. Tietze, on *Pathogenetic Outlines of Materia Medica*. These are important works, valuable alike to the student and the busy practitioner.

Before proceeding to the introduction of our special subject I must notice briefly a very important communication, addressed to this bureau by the American Ophthalmological and Otological Society, reading as follows:

BUFFALO, N. Y., July, 1879.

To the Chairman of the Bureau of Materia Medica, Pharmacy, and Provings, in the American Institute of Homœopathy, JABEZ P. DAKE, M.D., Nashville, Tenn.

At the third annual session of the American Ophthalmological and Otolological Society, held at Fort William Henry Hotel, Lake George, June 24th and 25th, 1879, the following motion prevailed :

That a committee of three be appointed by the President for the purpose of conferring with the chairman of the Bureau of Materia Medica, Pharmacy, and Provings, in the American Institute of Homœopathy, with the view of perfecting the ophthalmic and aural examinations, during the proving of remedies.

In fulfilling the spirit of this motion, the committee would suggest to the bureau the advisability, should it meet your approval, of having careful examinations of the eye and ear made by specialists, before, during, and after the action of the drug ; the former, to determine the condition of the visual function of the fundus, of the accommodation, of the refraction, and of the extrinsic muscles ; and the latter, to show the state of the external auditory canal and *membrana tympani*, with a careful record of the hearing power.

All of which is most respectfully submitted.

F. PARK LEWIS, M.D., Buffalo,
H. C. HOUGHTON, M.D., New York,
W. H. WOODYATT, M.D., Chicago.

This communication, coming into my hands after the adjournment of the Institute at Lake George, too late for submission to the bureau, I had it published in some of our journals, with comments, in order the sooner to draw the attention of the profession to its important hints.

The high standing of the society sending us this appeal, as well as the great importance of the object aimed at, calls for more than a passing notice. If practitioners, dealing with affections of the eye and ear, believe in the homœopathic principle, it is but right and necessary that they should seek to know exactly how the various articles of *materia medica* influence the conditions and functions of the human eye and ear. If they have found improved means of diagnosis in the study of diseases of those organs, it is but right and necessary that they should have such means applied in the diagnosis of corresponding drug affections.

If they require a *similimum* it is of prime importance that the *similimum* should be of the highest order, a well-ascertained fact and not a creation of fancy.

What they ask for, and what is needed by every practitioner under the homœopathic law, is the removal of the work of drug proving from the field of the busy practitioner, from the hands of the untaught student and the enthusiastic layman, to the physiological laboratory, where trained provers, male and female, under the guidance and scrutiny of expert observers, may furnish a faithful reflection of drug influence in the human organism.

Hahnemann did what he could, with the means at his command, towards building up a genuine and sufficient drug pathogenesis; and we are happy, in this large assemblage of his disciples and in these triumphal days of homœopathy, to bear witness that, with all its imperfections, the *Materia Medica* of the master and of his followers is far ahead of anything bearing the name *Materia Medica* that the world has ever seen. So valuable has it been considered by those who write textbooks for the old school of medicine, it has been freely drawn upon by them; but, much to the shame of the borrowers, without acknowledging, in preface or by footnote, the source of the wealth obtained.

So increased has become the appetite of our traditional adversaries for pathogenetic knowledge, it becomes us to look well to our laurels, lest by the adoption of the more exact methods of the day, they may cause the glory of the *healthy vital test* to depart from us and to adorn the experimental laboratories of their seats of medical learning.

Let us heed the call of our brethren of the Ophthalmological and Otological Society, and inaugurate more thorough methods of drug experimentation.

SPECIAL INTRODUCTORY.

In the meeting of the Institute at Lake George, last year, owing to the great number of papers in the several bureaus seeking to be heard, it was not possible for this bureau to submit its full report.

Under the circumstances it was thought best to reserve the

last two divisions of our subject for consideration during the year and presentation at this meeting. The divisions so deferred embraced the proofs of drug presence and efficacy in attenuations above the 6th decimal.

For convenience and the more efficient handling of our subject we made sections and assignments of our work as follows :

I. *The Proofs of Drug Presence and Power in Attenuations above the Sixth Decimal :*

1. As Furnished by the Tests of Chemistry : W. L. Breyfogle, M.D.

2. As Furnished by the Spectroscope and Microscope : C. Wesselhoeft, M.D., J. Edwards Smith, M.D.

3. As Furnished by the Tests of Physiology : T. F. Allen, M.D., Lewis Sherman, M.D.

4. As Furnished by Analogy from the Field of Impalpable Morbific Agencies : J. P. Dake, M.D. ,

II. *The Proofs of Medicinal Presence and Efficacy in Attenuations above the Sixth Decimal :*

1. As Furnished by the Tests of Clinical Experience in the Use of Attenuations, Ranging from the Sixth to the Fifteenth Decimal : J. F. Cooper, M.D.

2. As Furnished by Clinical Experience in the Use of Attenuations, Ranging from the Fifteenth to the Thirtieth Decimal : A. C. Cowperthwaite, M.D.

3. As Furnished by Clinical Experience in the Use of Attenuations above the Thirtieth Decimal : C. H. Lawton, M.D., H. M. Paine, M.D.

This *schema* of our work we had printed months ago in circular form and sent to all of our journals, most of which gave it an insertion and favorable notice. In order to gather from the observations and experiences of the profession at large, and of the scientific world outside, such facts as might aid our inquiries, we had the following appeal added to our *schema* and sent out in all directions :

“ The bureau will be pleased to receive items of information and experimental aid from members of the profession, and also from scientific persons outside who may be interested in any division of our subject.”

In regard to the constitution of the bureau itself and the subject selected for consideration I should say that, in but one instance, have we been called in question. Our motives have been somewhat impugned and our fairness questioned, with some detriment I fear to our endeavors to gather for this occasion all possible proofs of the power and efficacy of our drug attenuations.

In regard to the third section of our second division, embracing "Proofs Furnished by the Tests of Physiology," I should mention that we shall not be able to present all that we had hoped at the beginning of the year. It will be remembered that, at Lake George, Dr. T. F. Allen declared before the Institute, *that if six bottles of liquid, to all appearance exactly alike, one only being medicated, and that with the thirtieth centesimal dilution, were submitted to him, he would pick out the medicated bottle by the effects of its contents upon the human organism; and, further, that he believed he could thus pick out the thirtieth as often as the advocates of the low attenuations could the sixth under like circumstances.* Taking the doctor as in earnest I placed him upon the bureau, in order to have him apply the test proposed, in the development of proofs for this occasion, proofs so well guarded as to be above question.

I asked Dr. Allen and Dr. Sherman to take charge of all physiological experiments,—the former, of those pertaining to the 60th, and the latter, of those pertaining to the 6th decimal.

I requested Dr. Sherman to submit such safeguards for the preparation, marking, and care of the bottles to be furnished to Dr. Allen as would preclude all doubts as to the entire fairness of his test, and at the same time I requested Dr. Allen to do a similar work for the security of the experiments to be conducted by Dr. Sherman.

But, in place of going on with the work as laid out, Dr. Allen resorted to the President-elect of the Institute for a special commission to take charge of the preparations for his test, as though this bureau were incompetent or unwilling to deal officially and fairly with his undertaking.

Afterward, when brought to see his mistake, he declined the experiments entirely for the year, upon the ground of impaired health.

I mention these facts simply to account for the absence of results earnestly looked for by the profession, in view of Dr. Allen's proposal at Lake George, and the subsequent announcements made by this bureau.

Dr. Sherman, under rigid safeguards, went forward with the 6th decimal, developing proofs which, I am sure, will be gratifying to all who have believed that there is medicinal power in attenuated drugs.

In advance of the papers now to be read I must beg the earnest attention of all present to each fact and argument submitted, so that no misapprehension may occur as to the purposes and spirit of this bureau relating to the questions before us.

Some of the discussions which followed the reading of the papers of this bureau, last year, exhibited a wonderful misapprehension as to the subjects brought forward and positions assumed in them. In place of showing any errors of statement, or weakness of logic on the part of the authors of the papers presented, and, so far from shedding any additional light upon the matters under consideration, an attempt was made to lecture members of the bureau upon their supposed heterodoxy and their recreancy to the teachings of Hahnemann.

On this occasion I beg those who discuss our papers to keep in mind the real questions presented, and not to run off wildly, under a sense of horror or fear, lest the great temple of homœopathy be overturned.

In matters of science there should be a calm listening and looking to statements made, with a freedom from prejudice and a disposition to accept the truth, however much it may contradict what we have previously cherished as true.

All appeals to passion or prejudice, all closing of doors to full and free investigation, and all shrinking and hiding from the light, are unbecoming the members of this learned body, the cause we would further, and the age in which we live.

Not a member of this bureau has desired to suppress facts or discolor the truth, however earnest in the advocacy of cherished views. Not one has had a wish to weaken the foundation or mar the superstructure of homœopathy while combating the views of others.

We do not all agree—upon some points we differ widely—but I am persuaded we differ honestly, and with sentiments of personal respect for each other.

In closing my introductory to the reports now to be submitted, I beg leave to say that, as the senior member of this bureau, having served upon it almost constantly for a quarter of a century, and during these two years as its chairman, I must be allowed to retire from all official work in connection with it.

The duties of these two years have been performed by me with the determination of retirement and a purpose to have my last work well done.

*THE PROOFS OF DRUG PRESENCE AND
POWER IN ATTENUATIONS ABOVE THE
SIXTH DECIMAL, AS FURNISHED
BY THE TESTS OF CHEMISTRY.*

BY WILLIAM L. BREYFOGLE, M.D., LOUISVILLE, KY.

As you are all aware, gentlemen, the *science of chemistry*, as applied to homœopathic dilutions, cannot be proof conclusive of the existence of medicinal power, but as it is to become an important part of the proceedings of this bureau, I have endeavored to give the subject a thorough test. For this purpose I obtained on the 29th day of last August, from a reliable homœopathic pharmacy, two ounces each of Arsenicum 3d, 6th, 12th, and 30th decimal dilutions; of Nux vomica 3d, 6th, 12th, and 30th, and of Sulphur 3d, 6th, 12th, and 30th. The seals were left untouched, but the labels were removed, and in their stead numbers were placed on each vial and a corresponding entry made in a private book.

The vials were then turned over to J. P. Barnum, M.D., analytical chemist, with the understanding that each should be separately examined, using new apparatus for each test, and under the direct supervision of Professor C. Leo Mees and myself. No mention was made of the contents, excepting that Nos. 5, 6, 7, and 8 contained vegetable, and the rest mineral substance.

Test No. 1.

No. 1 (representing Ars. 3^x dil.) was then acidulated and treated, according to the usual method for determining unknown mineral substances, with no result until saturated with sulphuretted hydrogen, when a yellow precipitate was thrown down. Redissolving and applying Reinsch's tests, the copper was coated

with a metallic film, which on being sublimated, deposited on the glass tube distinct octohedral crystals of Arsenious acid, with crystals of Sulphur and some resinous matter, the latter from the alcohol, and the Sulphur arising from the decomposition of the Sulphuret of arsenic. On further examination the liquid was found to contain no other metallic substance.

Test No. 2.

No. 2 (representing the 6th dil. of Ars.) was then similarly treated. The metallic copper was slightly stained, and on being sublimated, Sulphur, resinous matter, and a few *indistinct* octohedral crystals were shown, not sufficient in number or distinct enough in form to determine their nature. Suspecting that they did not arise from the substance under examination, perfectly pure *distilled water* was similarly treated, and similar deposits of Sulphur and crystals occurred, showing that there had been a slight trace of Arsenic in the Muriatic acid used.

A solution containing $\frac{1}{50000}$ th of a grain of Arsenic was then similarly treated, and the copper was covered with a distinct steel-colored tarnish, which, on sublimation, yielded distinct crystals.

A solution containing $\frac{1}{100000}$ th of a grain of Arsenic was next treated. The copper was slightly tarnished and yielded minute but distinct octohedral crystals of Arsenious acid.

It was then concluded that No. 2 contained only alcohol and water.

Tests Nos. 3 and 4.

Nos. 3 and 4 (representing the 12th and 30th dil. of Ars.) were then each separately examined (using a fresh supply of acid, which showed absolutely no arsenical reaction) and no traces of anything but alcohol and water discovered.

The limit to which a chemist is willing to certify, as to the presence or absence of Arsenic, is from $\frac{1}{50000}$ th to $\frac{1}{100000}$ th part of a grain, yet a *crystal* measuring $\frac{1}{100000}$ th of an inch, and weighing less than $\frac{1}{100000000}$ th of a grain, may be distinctly recognized.

No such crystals were found in the 6th, 12th, or 30th dilutions, the 3d dilution alone showing traces of Arsenic.

As the substances contained in the series 5, 6, 7, and 8 were understood to be vegetable matter, they were carefully examined for the alkaloids.

Test No. 5.

No. 5 (representing Nux vom., 3d dil.) was evaporated slowly to dryness, dissolved in a small quantity of water, acidulated with acetic acid, and subjected to the color-test, and the absence of any known alkaloid determined.

The reaction of more than fifty of these substances are known, and solutions containing $\frac{1}{300000}$ th of a grain of Strychnine gives perfectly distinct reaction, while the $\frac{1}{100000}$ th part of a grain yields a fine display of colors.

Test No. 6.

No. 6 (representing the 6th dil.) was slowly evaporated to dryness, redissolved in a small quantity of slightly acidulated water, and injected into the circulation of a small frog, which was not in the least affected by it, while on the injection of the $\frac{1}{100000}$ th of a grain of Strychnine the animal was thrown into convulsions within six minutes, and died immediately afterward.

Considering the relative weight of the frog and the human subject, the administration of $\frac{1}{100000}$ th of a grain of Strychnine to a frog would be about equal to the administration of four grains to a human subject weighing one hundred and forty pounds; and the injection of the active principle in the contents of vial No. 6 equals the injection into the human system of the active principle contained in $43\frac{3}{5}$ gallons of water.

Tests Nos. 7 and 8.

Nos. 7 and 8 (representing Nux vom., the 12th and 30th dil.) were together concentrated by slow evaporation, slightly acidulated, and injected into the body of a frog weighing twenty-five grains, who was to all appearances not affected by it.

On the second day a solution containing $\frac{1}{200000}$ th of a grain of Strychnine was injected into the same animal, which showed symptoms of poison in a few minutes, and on agitation was thrown into a violent spasm and died soon afterwards.

From the same data, as given in test No. 6, this would have

been equal to the injection into the human subject of the active principle contained in $91\frac{1}{16}$ gallons of the solution contained in vials marked 7 and 8. Or if taken into the stomach, allowing the usually accepted ratio of 3 to 1, *equal to 273 gallons and 24 ounces.*

From the above experiments I am satisfied that the series contained no known principles of vegetable origin except alcohol.

Test No. 9.

No. 9 (representing Sulphur, 3d dil.) was tested according to the several methods for discovering unknown substances in solution, and slight traces of Sulphur were found. The contents of the whole bottle was then evaporated to dryness, treated with boiling Nitric acid, slightly diluted with water, and a solution of Chlorate of barium added, a faint white precipitate of Sulphate of barium was thrown down, barely discernible to the naked eye, but showing conclusively that Sulphur was the substance in solution.

Tests Nos. 10, 11, and 12.

Nos. 10, 11, and 12 (representing the 6th, 12th, and 30th dil.) were then similarly treated and gave no traces of Sulphur, containing only alcohol and water.

By the above method the presence of $\frac{1}{40000}$ th part of a grain of Sulphur can be readily detected, and acid containing $\frac{1}{20000}$ th part of a grain of Sulphur will cloud the solution and show a distinct precipitate.

As before remarked, in each of the above experiments, which extended over a period of three months, new apparatus was used, and every possible precaution taken to prevent the introduction of foreign matter, and to secure accurate results.

Although the tests, as applied above, failed to discover any vegetable substances in the third dilutions, and no traces of minerals above the 3^d, I determined to push these experiments still further, and, if possible, not only to demonstrate their curative power, but to discover how much of a given substance would produce an *aggravation* of the symptoms.

My first case was one of continued "morning sickness;" the patient being unable to take even water without vomiting. Had

been under allopathic treatment for six weeks, and abortion was recommended. I tried internally, Lactic acid, Ipecac., Sepia, and Nux moschata without perceptible change. Remembering that in test Nos. 6 and 7 the injection of the active principle of 2 ounces of Nux vom. 6^x dil. into a small frog produced no effect, and that this equalled the effect of 273 gallons and 24 ounces taken into the human stomach, I felt justified in adopting a new method. Procuring a new hypodermic syringe I carefully injected under the skin of the forearm ten drops of Ipecac., the 6^x dil. The effect was magical. In a few hours my patient was enabled to take food, and speedily recovered without even a repetition of the dose, or the slightest aggravation of the symptoms. Two eminently respectable homœopathic physicians of Louisville can *substantiate* the above statement.

Emboldened by the successful results in the above case I next injected ten drops of the 6^x dil. of Tartar emetic, in the case of a policeman suffering from a violent attack of asthma. The paroxysm was relieved in twenty minutes, and the patient enabled to lie down and sleep for the first time in three days. This was repeated four times, at long intervals, and the patient speedily recovered. At no one time did we discover the slightest aggravation of the symptoms following its use.

The next case was one of dysmenorrhœa, and the violent colic yielded in less than ten minutes to the hypodermic injection of ten drops of Pulsatilla 6^x dil. over the region of the uterus.

In more than fifty cases, where this plan of administering medicines was used, in not one single instance was there the slightest apparent medicinal aggravation, and although the dilutions were all used crude (that is, undiluted with water), in no case did the alcohol produce a sore, unless used in *fleshy* parts, and then in but very few cases.

The conclusions drawn from the above experiments have convinced me (had such been necessary) of the efficacy of dilutions above the 6th decimal, and that, although beyond the reach of the tests of chemistry, they still contain medicine enough to cure disease.

It has also convinced me that homœopathic medicines, hypodermically administered, are often of great value.

It has strengthened my confidence in the administration of the single dose, *provided* that the dose be sufficient to at once control the conditions.

It has shaken my faith in so-called *aggravations* from homœopathic dilutions, above the 6th dil., and I am led to believe that this is a question but imperfectly understood; that the profession rely too much upon prejudices unsustained by facts.

It is my opinion that this *fanatical search* after *new attenuations*, implies a want of confidence in the material of the present. To say that the 30th dil. will cure where the 6th dil. will not, is to deny the truths discovered by Hahnemann. To say that it makes a *better* cure still implies a *doubt*. To say that it is because of medicinal aggravations, is to state what is not capable of verification.

Experiments with the different attenuations are legitimate; but as they in no wise affect the principles of homœopathy, we should, as far as possible, confine their scope within the boundary of well-established scientific methods. In our great work of reforming medicine we ought not to envelop the plain, simple, all-convincing truths—our powerful weapons—in a mystic, psychological halo, which is, to say the least of it, not properly understood by our own profession.

It retards our own progress and causes the scientific investigator to hesitate.

REMARKS AND SUGGESTIONS CONCERNING CERTAIN HOMŒOPATHIC TRITURATIONS.

BY J. EDWARDS SMITH, M.D., CLEVELAND, OHIO.

It will be remembered that, at the last session of this Institute, held at Lake George, I had the honor of presenting a paper discussing, to some extent, the behavior of Aurum under the pestle, as developed by the process of trituration.

This paper, as presented, was simply a narration of a series of microscopic observations on various triturations of Aurum. It was accompanied by a series of explanatory drawings, made directly from the microscope, and by the aid of the camera-lucida.

Before leaving the Lake I had formed the determination to still further pursue these investigations, with the hope of eliminating any error existing in my past work, or perchance of arriving at new facts of interest to the profession.

During the vacation the study of the triturations has received all the attention that circumstances would permit; one drawback alone will here be mentioned, viz., my eyes will no longer bear the severe strain consequent on the protracted sitting to which they have so long been accustomed. Thus it has often occurred that my daily use of the microscope professionally was all, nay, even more than the state of my eyes would allow. Nevertheless, these microscopic, as well as other, observations, have, however, been made in the interim, some of which, at the date of this writing, are still occupying my attention, the results of which will be presented at a future session.

In laying out my work of the past season I determined to review carefully the ground covered by my original contribution to the Institute, desiring thereby either to confirm my past ob-

servations, or, on the contrary, as I have already intimated, to expose any latent errors perchance therein contained. It is always more pleasant to discover one's own blunders than to accept the tender mercies of the critic.

Knowing full well the paramount importance of the proper illumination of an object under the microscope, especially when triturations of Aurum are examined, I have expended considerable time and study in this direction. I now have to report that the method of using the solar beam in connection with the use of the high-apertured objectives, suitably diaphragmed, as described by me at Lake George, I still find to give me results decidedly superior to all others I have tried, especially as to the *recognition* of gold.

This matter of the *recognition* of gold, during examinations of its triturations, is of the utmost and vital importance. It will be remembered that at the last session I pointed out the fatal errors likely to occur from the use of reflected light; nevertheless, it is to be noticed that, during the past year, one observer at least, engaged in the study of gold triturations, used this same reflected light constantly, to the almost exclusion of all other methods of illumination.

I have also reviewed that portion of my past work which was accompanied by the series of drawings executed by aid of the camera-lucida and microscope under amplifications of 1500 diameters; my object being, first, to ascertain whether certain infinitesimally small particles, which, under amplifications of 1500 diameters, were too dimly seen to be recognized as gold, or to be accurately measured, might not be, under higher powers; in which case to also measure the same as accurately as possible. In this work the chief instrumentation consisted of:

1st. A superb duplex immersion, one-tenth, by Tolles, balsam aperture of 100 degrees. This objective displays all the known tests, including the Fassolt 120,000th band of lines.

2d. A special micrometer stage-plate, ruled especially for me by Professor William A. Rogers, of Cambridge, Mass. This plate contains the standard American inch, and also the French centimeter. A horizontal line traverses this plate from right to left. Above this the standard inch lines are ruled, while below are

the lines of the centimeter; both sets of lines contact the horizontal one, hence it occurs that the two sets of lines at the point of contact with the horizontal are seldom coincident, the one series forming "laps" with the other. Now by measuring these "laps" under the objective, and comparing the said measurements with their tabular values, we have the means of cross-questioning the accuracy of the rulings in the severest manner possible. The plate in question, under this close scrutiny, will always give me values true to three places, and often four places, of decimals. This plate is a marvel of mechanical skill.

3d. A Donald cobweb micrometer, fitted with one-fourth inch ocular, the value of the wheel divisions being determined by aid of the Rogers's stage-plate above mentioned.

4th. Tolles's achromatic amplifier. This instrument doubles the power of the objective and eye-piece, without disturbing the corrections of the objective.

5th. Tolles's patent one-half and one-fourth solid oculars.

6th. The new "Acme" stand, the joint production of Mr. John Sidle and myself. This stand combines all the latest improvements. For the close work contemplated the camera-lucida is not adapted, and was not used.

It will be remembered that my paper read at Lake George contained an account of Mr. Witte's new triturations of Aurum from the precipitate by phosphorus in ether. These, being so far superior to all others I have met with in point of fineness of the gold particles therein contained, were selected for the work in hand. Of these the 2d, 3d, and 6th decimal received my special attention. Further along I shall have occasion to again refer to these superior triturations.

The method of mounting the triturations was the same as that described last year, and the superiority of the method further demonstrated, as it seems to me, beyond a reasonable doubt.

I now proceed to give briefly the results of my observations over the new triturations of Mr. Witte, above mentioned.

These I have examined time after time, using the transmitted solar beam in conjunction with the diaphragmed objective, the greater portion of my time and attention being devoted to the 3d and 6th decimal triturations.

With the objective in perfect adjustment, and by the most delicate manipulations of the illumination, I have discovered, under amplifications of 2000 diameters and upwards, certain exceedingly small particles which exhibit the characteristic behavior of gold. These minute particles, after increasing the amplifications to 4000 diameters and upwards, were carefully measured with the cobweb micrometer, and found to vary between $\frac{95}{10000}$ to $\frac{115}{10000}$ of an inch. The most diligent search, coupled with the most delicate manipulations, failed to display any smaller particles than the above named, having the slightest appearance of metallic gold.

I desire to call your attention to a fact, to which many of those before me can attest, viz.: At the last session of this Institute, at Lake George, the writer placed on record his utter disbelief as to the capacity of the microscope to determine the ultimate divisibility of matter. The question with the author from the outset has been, how far—to what extent—is the microscope of service in enabling us to study the behavior of drugs under the pestle during the processes of triturations.

Leaving then the item of ultimate divisibility out of consideration, the question arises, what has the microscope taught concerning our triturations; or, referring to my own investigations, what has the author learned from the microscope concerning triturations of Aurum, during the past two years, which were not previously known and accepted.

To this interrogatory I responded as follows:

First. A certain so-called trituration, labelled and sold for Aurum 3x, contained no gold at all.

Second. Mr. Witte's trituration of Aurum foliatum has been demonstrated to be almost equal in fineness of particles to the average triturations from the precipitate.

Third. Four-hour decimal triturations are not very far superior to two-hour triturations.

Fourth. Triturations of Aurum met., up to the 6x, from various makers, vary considerably; no two being identical in fineness of the contained particles.

Fifth. The popular idea that particles of gold are ten times smaller in the 2d decimal trituration, than in the 1st decimal, is

very far from being correct. Nor are particles of gold in the 3^x ten times smaller than in the 2d decimal trituration. These are plain facts and are easy of demonstration.

Sixth. In all the triturations of gold, from the 1st to the 6th decimal, examined by me, fully 33 per cent. of the metal escapes subdivision under the pestle, *i. e.*, does not become subdivided to anything like the extent formerly accepted.

Seventh. It is quite possible with careful manipulations to display particles of metallic gold under the microscope which, in point of minuteness, challenge our most difficult test-objects.

Among the most difficult test-objects known in modern microscopy may be quoted the finest bands, ruled by Professor Rodgers, of Cambridge, Mass., and Mr. Charles Fassolt, of Albany, N. Y. Either of these equal 120,000 lines to the inch. To display these delicate rulings the utmost delicacy of manipulation, coupled with the employment of modern objectives possessing exquisite definition, is imperatively demanded; furthermore, it may be asserted that few, even of the modern opticians, furnish object-glasses equal to the task.

The finest object-glasses capable of resolving the delicate tests above named are, and for the past eight years have been, made in the United States. The work of the veteran, Mr. Charles A. Spencer, of Geneva, N. Y., and that of Mr. R. B. Tolles, of Boston, Mass., is well known and appreciated, both at home and abroad. The late glasses of Zeiss, of Ger Denmark, are quite equal, in point of definition, to those of Spencer or of Tolles.

Exceedingly fine object-glasses have lately been produced by the Messrs. Bausch & Lomb, of Rochester, N. Y., and also by Messrs. Powell & Lealand, of London, England.

In giving, as above, the names of the several opticians whose objectives alone exhibit that exquisite definition rendering them successful to attack the most difficult problems, in the line of test-objects, known in microscopy, *i. e.*, the resolution of the Rodgers and the Fassolt 120,000th bands, the seeming digression may be pardonable, when it is taken into consideration that observers using object-glasses, known to be of inferior quality, sometimes claim results which would at sight be rejected by microscopic experts. One author, during the past year, claims to

have seen particles of gold measuring but $\frac{1}{5000}$ th of the m.m., $\frac{1}{12700}$ th of the English inch, and this, too, with reflected sunlight. Comment is here unnecessary.

Returning to the enumeration of the results possibly accomplished by my own observations, I have now to call your attention to a most interesting and, it seems to me, important matter, which has been brought to light, and to a certain extent developed during my studies of the gold triturations.

Referring back again to my original paper, read at Lake George, many of those present will recollect that I exhibited a specimen of a new trituration of Aurum metallicum, prepared by Mr. L. H. Witte, pharmacist, of this city, to whom I am under great obligations for his valuable assistance from the commencement of my observations to the present date.

At our last session I was content with simply inviting your attention to the new preparations. You were shown that the 1st decimal trituration was very much darker, even approximating to the color of slate, as also that the 3d decimal trituration was even darker in color than the usual 1st trituration. Accompanying the statement that these new triturations of Mr. Witte's were prepared from gold thrown out of solution of the chloride by the addition of ether and phosphorus, I also asserted that the particles of gold thus thrown out of solution average smaller than those found in the usual 1st, 2d, and 3d decimal triturations.

On my return home, Mr. Witte, at my request, carried his new triturations up to the 14th decimal.

While engaged in the study of these last triturations, I had occasion to dissolve a small quantity in a little distilled water, and was astonished to find that the liquid instantly assumed a clear and almost transparent purple color; adding a little alcohol by way of preservative the purple fluid remained constant and unchanged for weeks and even months.

Now, if to a solution of the chloride of gold we add, as did Mr. Witte, a solution of ether in phosphorus, we shall obtain this same purple solution, which, when examined under the microscope, will be found to consist principally of metallic gold, and in a state of very fine subdivision.

Having determined this fact it became desirable to recover this gold intact, and by some method render the process practicable to the homœopathic pharmacist.

To secure this desideratum Mr. Witte and myself devoted much time and attention, and to Mr. Witte belongs the honor of having discovered a process at once practicable as well as simple, and meeting the requirements of the pharmacist. This formula will be given in detail before concluding this paper.

If, after recovery, the gold be submitted to the process of trituration, and subsequently dissolved in water we have, as I have before stated, the original purple liquid again, that resulting from the trituration being obviously purer than that occurring from the addition of ether in phosphorus to the solution of the chloride.

If, after having dissolved a little of the trituration in water, adding alcohol, as before stated, and allowing the purple fluid to remain quiet, say for ten days, a light sediment will be noticed at the bottom of the vessel. By decanting now the supernatant purple liquid into a clean bottle no further sediment seems to form at least for weeks; here we have the purple liquid in its purest form.

As to the nature of the sediment referred to, I opine that it consists principally of the impurities contained in the sugar of milk, and perhaps of a small proportion of the larger particles of metallic gold. In this direction there is still room for further study.

If we attempt to dissolve too much of the trituration we may obviously expect a sediment.

The clear purple fluid, after a greater or lesser period, say from two to four months' time, will finally settle to the bottom, leaving the liquid above as clear as water. If, however, we shake the bottle slightly this precipitate disappears, and the purple fluid again makes its appearance.

Any attempt to examine the gold contained in the purple fluid in the wet state will fail. This is true of the usual test-objects used by advanced microscopists. By evaporating, however, a few drops on a glass slide the slide becomes gilded, having the appearance of watered silk described in my former paper.

These new triturations present peculiar claims for your consideration.

1st. Their very much darker color. This to the professional pharmacist will be a matter of interest and likely to excite his attention, knowing full well that the finer the comminution of particles the darker will be the resulting color of the trituration.

2d. No previous trituration of gold will give the purple fluid when dissolved in water.

3d. The lower triturations, say the 1st and 2d, are easily demonstrated by the microscope to be far superior to previous triturations, and, be it known, that on the integrity of the low triturations rest that of all the higher ones.

I now proceed to dissolve samples of these new triturations in water, and will pass them around for your personal inspection (specimens prepared and passed to members). With these you will also receive a small sample of the 1st decimal trituration, dry, for your further examination at your leisure (samples of the trituration here given to members present).

The 5th decimal trituration dissolved in water will communicate to the latter a perceptible purple tinge. Experienced eyes, accustomed to judge of colors, might detect the color in the higher triturations.

The question now naturally arises, does this purple fluid contain gold in suspension or gold in true solution?

As proof that this gold is truly in a state of suspension the following experiment may be cited:

Resin dissolved in pure alcohol gives a perfectly clear and transparent liquid, which cannot be distinguished by the eye from alcohol without the resin. To this solution add water very carefully until the resin shall be *very slightly* thrown out of solution. If the experiment be conducted properly the resinous particles, in a finely divided state, will remain suspended in the liquid permanently, while the latter will exhibit a trace of color. We know now from obvious reasons that the resin is not in a state of solution.

Again, it is a well-known fact that certain solutions of silver are liable to turn red owing to the reduction of the metal in a finely comminuted state; the reduced silver being held in suspen-

sion cannot be removed by filtering. If, however, to the liquid be added a suitable quantity of Kaolin, and the whole well shaken and immediately filtered, the reduced silver will be separated, the silver remaining on the filter, and in this manner it may be recovered.

Now, as may be hereafter seen, this shaking with Kaolin and subsequently filtering is a process practically identical with that adopted by Mr. Witte for the recovery of his gold.

I may here remark, *en passant*, that the superiority of gold, in point of fineness, precipitated from solutions of the chloride by phosphorus and ether, was clearly demonstrated by microscopic examinations, and hence the importance of recovering this gold for the purpose of subsequent triturations.

Furthermore, from the minute particles thus thrown out of solution by the action of ether and phosphorus, together with the presence of the purple color, may it not be urged that the metal is simply held in the vehicle in a state of true suspension?

In support of this position many natural phenomena may be cited, *e. g.*, the prevailing blue of the heavens, the green of the ocean wave, both owe their color to myriads of infinitesimal particles held in true suspension. The gorgeous tints seen on the feathers of the peacock depend on the close striation of particles. This latter fact is easy of demonstration under the microscope by separating the particles under a low power, whereupon the beautiful colors vanish, being replaced by a dingy brown.

Again, the fine bands of the microscopist's test-plates all show a characteristic color when examined by reflected light.

Nor are the results of my microscopic examinations of these new triturations such as would strengthen the hypothesis of the gold being in a state of true solution, for, in every instance, the *mass* of gold seen was not in particles smaller than $\frac{1}{60000}$ th of an inch. The very finest particles alone were those measuring from $\frac{1}{95}$ th to $\frac{1}{115000}$ th of the inch.

Finally, as I have already stated, if we allow the purple fluid sufficient time the gold will eventually settle to the bottom of the vessel, leaving the supernatant liquid entirely devoid of color.

During the past few months samples of the new triturations

have been sent to friends prominent in our profession for examination and study. The formula for their preparation has, however, been withheld for the purpose of presenting it to the Institute during its present session. Nearly all of those to whom the triturations have been forwarded seem to regard the gold as in a state of solution. A letter received by me, bearing date of June 2d, from a gentleman favorably known to all before me, contains the following paragraph :

“One thing, however, strikes me about this preparation, and that is, that it can hardly be said to be in suspension, but that it is truly in solution. According to chemical authority, Silliman, ‘solution is the result of a feeble affinity, but one in which the properties of the dissolved body are unaltered;’ and according to Henry, ‘solution is a state of diffusion of the particles of a solid throughout a liquid, without destroying its transparency.’ As your new preparation of gold comes fully under the conditions just quoted it will be considered as soluble, and not merely suspended. That, of course, will not affect the value of the discovery.”

Other letters received during the past three months from intelligent and valued correspondents are similar in tenor to that just quoted.

Be the facts what they may, there will be plenty of time in the future to arrive at a verdict, and, after further deliberation and study. For the present I am content thus to leave the question open.

Of the superiority of these beautiful triturations there can be no manner of doubt. Furthermore, this marked improvement appeals with force to those using high as well as low potencies.

Is it probable that all the remaining triturations have reached the acme of perfection, and may there not be room for their further study and improvement?

In conclusion I desire to place on record my obligations to Mr. Witte for the valuable assistance he has rendered me during the entire progress of my investigations, and I feel assured that when his new triturations shall become better known that the profession will not be backward in their appreciation of his valuable discovery, which he gladly presents through this In-

stitute to the profession at large. His formula is as follows, which I give in nearly his own words :

Sixteen grains of gold were dissolved in nitro-muriatic acid ; to this solution sixteen pints of distilled water were added.

Six grains of phosphorus were dissolved in twelve fluid ounces of ether. The two solutions were mixed together.

The action of the phosphorus upon the chloride caused its reduction, there resulting phosphorus compounds and metallic gold, the gold in particles so fine as to remain in suspension and to pass through the filter-paper with facility.

To separate the gold a solution of albuminum chloride was first added, then aqua ammonia in excess. The action of the ammonia caused the formation of albuminum hydrate, which, when filtered out, retained the particles of gold. The bulk of the albuminum hydrate upon the filter was about one cubic inch.

The albuminum hydrate was now dissolved out with muriatic acid, the solution passing through the filter and leaving the gold, which was then thoroughly washed with alcohol to dissolve out any phosphorus, and then with distilled water until nothing but the pure gold in fine particles was left upon the filter. The gold thus remaining was subsequently submitted to the usual process of trituration.

*PROOFS OF DRUG PRESENCE AND POWER
IN ATTENUATIONS ABOVE THE SIXTH
DECIMAL,*

AS FURNISHED BY THE SPECTROSCOPE.

By C. WESSELHGEFT, M.D., BOSTON, MASS.

THIS is the work allotted to me for the Bureau of Materia Medica, Pharmacy, and Provings. The problem appears large, and its solution seems to promise more than we shall actually be able to realize.

The spectroscope is, in the minds of many, a somewhat marvellous instrument, which is thought to reveal to us, not only hidden secrets, but also to afford direct proof of the infinite divisibility of matter.

Revelations to unaided senses, of extremely minute portions of matter, such as the odor of fragrant flowers perceived at a great distance, or the perception by the senses of infinitely minute quantities of metals, seems less marvellous than when revealed to us through the medium of some instrument, such as the spectroscope, or the transmission of words to a great distance by the telephone.

In order to arrive at a clearer understanding of what we may actually expect from the spectroscope, a brief sketch of its inception and progress is in place, and for this there is no better authority than Mr. Henry E. Roscoe's *Lectures on Spectrum Analysis*.

The first principle to be understood in this connection is that when light is allowed to fall through a prism against a screen, it will be perceived to have been separated into its component parts, and will appear split up into a band exhibiting the colors of the

rainbow. This is the solar spectrum. None of the different colors allow of further splitting up into other colors, and each differs from the other in wave-length, and intensity of vibration.

Besides the visible rays of light, there are invisible rays, playing a most important part in the nature of solar light. The red rays produce the most heat; the yellow rays are the most luminous to the eye; while the blue and violet possess the greatest chemical action. (*Photography*.) The dark heating rays are invisible, but demonstrable by Tyndall's experiment with the ray-filter, a solution of Iodine in disulphide of Carbon, by which all the luminous rays can be filtered out, and only the invisible heating rays are transmitted, which is proved by the ignition of paper, heating of platinum, etc. The solar spectrum is not a gradual transition of one kind of light into another, but interspersed between the shades and in them are certain dark lines, to be regarded as shadows, or spaces where certain luminous rays are absent (L. C. Wollaston, *Philosophical Transactions*, 1802, p. 328). They are only obtainable by allowing the light to pass through a narrow slit. They are always the same in any kind of sunlight, direct or indirect, and their invariable position is known and carefully mapped out; because by knowing exactly the position of these dark lines in the solar spectrum, we can ascertain that iron, sodium, and other well-known substances exist in the solar atmosphere. This was discovered by Wollaston; they were first mapped out by Fraunhofer to the number of 576, in the year 1814, and are called after him.

Every chemical element, in a state of gas, when heated, gives off a peculiar light, so that the spectrum of every element in a state of glowing gas, is totally different from that of any solid body, inasmuch as, instead of a continuous spectrum, it presents a discontinuous one containing light bands or lines, indicative of the presence of the particular elementary gas in question.

Various bodies burned in a flame, *e. g.*, Lithium, Soda, or Potash, color the flame yellow and violet; Salts of strontium color the flame crimson; Barium green, etc., and each chemical substance can be detected by its peculiar light, when burned in any luminous flame like that of Bunsen's burner.

Every chemical element has the property of emitting its peculiar light, if its vapor can be heated so as to become luminous.

Most of the alkaline earths can readily be detected by merely burning them in a sufficiently hot flame; but the spectroscope is the instrument by which very minute quantities of each substance can be detected. This instrument consists of a prism placed on a disk or platform; a telescope is fixed on one side of the disk, and is directed towards a surface of the prism. On the opposite side of the disk is placed a tube having a lens in the end directed toward the prism, and a narrow vertical slit in the other end directed towards the flame. This form of spectroscope with a simple lens will answer the purpose of detecting the minutest particle of most substances. More complicated instruments, showing two spectra from two flames, are in use; also spectroscopes with many prisms, which have the effect of not only showing colored bands in the spectra of various substances, but which by repeated refraction, split the simple colored bands into masses of bright lines, each one as fine as the most gauzy spider's web.

As each metal, salt, etc., produces bands of peculiar color, always occupying the same place in the spectrum, maps have been prepared of the spectra of various substances, showing the exact position, breadth, and intensity of the bright lines, enabling observers to identify the presence of known metals, and to recognize the presence of unknown elements. The advantage of this method of analysis consists in its delicacy, which greatly exceeds the most delicate tests of chemistry. The manner of using the instrument, is to place a drop of a solution to be tested upon a small loop of platinum wire, and to burn it in a Bunsen burner in front of the slit of the tube, while the observer looks through the telescope at the prism, in a dark chamber. If metals are to be observed which are not volatilized in an ordinary flame, they are made incandescent by means of the electric spark, a method more difficult of application than the former, both on account of the additional apparatus, as well as owing to the rapid action of the spark.

Instances of the Delicacy of Spectrum Analyses.—The most sensitive reaction is that for Sodium, the minutest particles of which, unless previously guarded against, disturb all other tests. So-

dium is not only everywhere present, but soon returns if removed by heat, so that a thin platinum wire, if not used for a few minutes, will at once show the yellow sodium lines again, which must be removed when trying another substance on the same wire. $\frac{1}{1000000}$ th part of a milligram, or $\frac{1}{1000000}$ th part of a grain can be easily detected. It is always present in the air, and all bodies exposed to the air show, when heated, the yellow soda line.

Lithium, $\frac{1}{1000000}$ th part of a milligram, or $\frac{1}{1000000}$ th part of a grain can be easily detected by its red lines. It was formerly known to exist in only four minerals. It is now found by spectrum analysis to be one of the most widely distributed elements. It exists in all rocks, in the ashes of most plants, in milk, human blood, and muscular tissue. *Strontium* is easily detected by red and blue bands, when only $\frac{1}{1000000}$ th of a milligram, or $\frac{1}{1000000}$ th of a grain is present. *Calcium*, $\frac{1}{1000000}$ th of a milligram, or $\frac{1}{1000000}$ th of a grain of lime can be easily detected. *Cæsium* and *Rubidium* were detected in the proportion of two hundred grains in forty tons of mineral water.

Thallium and *Indium* were discovered in a similar way. To illustrate the delicacy of the spectrum reaction, I transcribe the following: "The following experiment shows that the chemist possesses no reaction which in the slightest degree will bear comparison as regards delicacy with spectrum-analytical determination of Sodium. In a far corner of our experiment-room, the capacity of which was about sixty cubic centimeters, we burnt a mixture of three milligrams of Chloride of sodium with Milk-sugar, while the non-luminous, colorless flame of the lamp was observed through the slit of the telescope. Within a few minutes the flame, which gradually became pale yellow, gave a distinct Sodium line, which, after lasting ten minutes, gradually disappeared.

"From the weight of Sodium burned, and the capacity of the room, it is easy to calculate that in one part by weight of air there is suspended less than $\frac{1}{200000000}$ th part of soda smoke, as the reaction can be observed with all possible comfort in one second, and as in this time the quantity of air, which is heated to ignition by the flame, is found, from the rate of issue, and from the

composition of the gases of the flame, to be only about 50 cubic centimeters, or 0.0647 gram of air, containing less than $\frac{1}{20000000}$ th of Sodium salts; it follows that the eye is able to detect with the greatest ease quantities of Sodium salt less than $\frac{1}{30000000}$ th of a milligram in weight.”*

The application to homœopathic pharmacy of the above observations is a very easy matter, and it appears at a glance that the quantities of matter observable by spectrum analysis, permitting the ocular perception of $\frac{1}{50000000}$ th part of a grain of Lithium, or $\frac{1}{150000000}$ th part of a grain of Sodium, does not exceed the 4th centesimal or 7th decimal dilution. But it must not be supposed that because we fail, by the spectroscope, to see the presence of matter any further, that it is not present in many subsequent dilutions, nor that modern science does not allow us to calculate the limit to which soluble matter may be carried by progressive dilution. As I have shown in my last report to the Institute (*Transactions*, 1879), modern molecular science demonstrates that, in accordance with the average size of molecules, their number is calculable in a given quantity of water, or some other fluid, and such calculation is derived from the fact that a volume of substance, when reduced to a liquid form, is not much greater than the combined volume of its molecules.

According to this calculation the 11th centesimal, or 22d decimal dilution, would be the limit to which matter can possibly be carried, which is fully corroborated by Mr. Crooke’s experiments on radiant matter. Hence, spectrum analysis is inadequate to demonstrate the extent of molecular divisibility to the senses.

In order to test the matter personally, I undertook the spectroscopic analysis of several substances (wherein I was assisted by Mr. Fletcher, Lecturer on Chemistry at the Boston University School of Medicine), at the Institute of Technology, where the use of instruments was most kindly placed at our disposal. I prepared decimal dilutions of Chloride of sodium and Sulphate of lithium as the two most available substances for analysis.

* L. C., from Kirchhoff and Bunsen’s first Memoir on Analysis by Spectrum Observ.—*Phil. Mag.*, vol. xx, 1860.

Each of these substances was diluted in distilled water in proportion of ten grains to one hundred drops of water, and carried up on this scale to the 8th decimal. The spectroscope was placed in a dark room; before its slit there was a Bunsen's burner, reinforced by atmospheric pressure to give a very intense flame.

Very thin platinum wires, melted into glass handles at one end, were bent into a little loop at the other end, just large enough to hold the fraction of a drop of the solution. Having burned off all the Sodium deposited by the atmosphere on the wire, it was then dipped into the 8th decimal dilution of salt, and held in the flame. This gave no spectrum with the characteristic yellow lines of sodium. These lines were faintly observed in a fraction of a drop of the 7th decimal, and very distinctly in dilutions below the 7th decimal.

Lithium sulphate was subjected to the same process, but showed no characteristic red lines above the 3d at these experiments; although a repetition, which I had no time to make, would undoubtedly have shown the presence of Lithium further, and I do not regard the last test as very conclusive.

But taking into consideration the more successful Sodium test, we find that the 7th decimal dilution contains the $\frac{1}{100000000}$ th part of a grain. Each drop of the 7th decimal would contain $\frac{1}{100000000}$ th of a grain, and when we consider that a minute fraction of a drop, perhaps only the tenth part, was burned in the flame, we must conclude that the test is one exceeding in fineness the one recorded by Bunsen and Kirchhoff, who say that $\frac{1}{100000000}$ th is easily detected, while our fraction of about a hundred-millionth in the 7th decimal was only less easily seen, allowing a brief flash of yellow bands to become visible more nearly within the limits of the statement of those observers.

*PROOFS OF MEDICINAL PRESENCE AND
POWER IN ATTENUATIONS ABOVE
THE SIXTH DECIMAL,*

FROM THE STANDPOINT OF THE PHYSIOLOGIST.

BY LEWIS SHERMAN, M.D., MILWAUKEE, WIS.

THE determination of the limits of medicinal power in attenuations must be made by experiment. Observation and analogy cannot give positive and unequivocal proof. They can, at best, give such indications as will point out the experiments which are necessary to this end. In observation the conditions are supplied by the natural course of events, and they have no designed reference to the question to be settled. In a perfect experiment, the conditions are designedly arranged so that only the force in question can determine the result. In searching for the limit of drug power in attenuation, three methods of experimentation have been pursued.

1. Experiments in which the provers took the medicinal preparations to be tested, and recorded all the sensations observed for several days or months afterwards.

2. Experiments in which the provers took Sugar of milk, and recorded all the sensations observed for several days or months afterwards.

3. Experiments in which the provers took the medicinal preparations at one time and the menstruum at another, without knowing, in any instance, which was the one and which was the other; and by close observation of their sensations tried to discover which was the medicinal preparation.

A typical example of the first of these three methods of experimentation is the "Reproving of Sepia," made under the

direction of this bureau, published in the *Transactions of the Institute for the year 1875*. The chairman of the bureau, the late lamented Carroll Dunham, selected the subject and requested the professors of Materia Medica in the homœopathic medical colleges to organize classes of provers among the students.

Thirty provers were secured, and thirty-five provings were made.

Ten provings were made with the 3d centesimal trituration, seventeen provings with the 30th centesimal trituration, and eight provings with the 200th centesimal dilution. The ten provers of the 3d trituration recorded one hundred and ten symptoms, the seventeen provers of the 30th trituration two hundred and twenty-two symptoms, and the eight provers of the 200th dilution three hundred and four symptoms.

The first prover, an unmarried lady medical student, took five powders of *Sepia* 200th, and experienced during the following winter seven quarto pages of symptoms. The most prominent among these were tenderness of the mammæ, distress in the pelvis, great vascular excitement, a sensation as if the vulva were enlarged, frequent micturition. There were numerous and various pains, dull, dragging, pressing, throbbing, lancinating, shooting, darting, gnawing, and bearing down.

Once during the proving the menses came eight days ahead of time. This happened sixty-one days after the taking of the last dose of *Sepia* 200th, and only six days after taking a dose of *Nux vomica*, low. Yet the symptom forms a part of the pathogenesis of *Sepia*. The most remarkable symptom of all, perhaps a "keynote," was a mental one, "I recall known facts by association." This also occurred long after the taking of the last dose of *Sepia* 200th, and soon after the taking of a dose of *Nux vomica*, low. Nevertheless it is incorporated in Dr. Dunham's Hahnemannian arrangement of the symptoms, and forms a part of the recorded pathogenesis of *Sepia*.

The second prover, also a lady medical student, took two powders of *Sepia* 200th. She suffered great mental depression, and urinated quite freely, passing a very large quantity. She had very severe pelvic pains, and her mammæ were very sore. Like

the first prover she felt very stupid during the whole lecture course, and had great difficulty in answering questions.

The symptoms lasted exactly one hundred and eleven days, and covered seven quarto pages of the *Transactions*. Their severity may be inferred from the fact that the word *very* occurs in the record more than seventy times.

Another prover, under the personal supervision of Dr. William Owens, of Cincinnati, Ohio, took a dose of *Sepia* 30° at nine o'clock in the evening. At four o'clock the next morning she became alarmed at her condition and took a dose of *Aconitum*, 3d. Before night her feet gave forth a most offensive odor. This symptom is incorporated by Dr. Dunham in the arranged pathogenesis of *Sepia*. After nine days the prover took another dose of *Sepia* 30°, followed in a few hours by a dose of *Aconitum* 3^d. Nine days later she observed a fine rash over the body, particularly about the bends of the elbows and knees, brought out by heat and caused to disappear by cold. This symptom is incorporated in the pathogenesis of *Sepia*.

Hungry symptomatologists assume that such provings as these represent the disease-producing powers of drugs. Careful students of drug-action believe that most, if not all, of the symptoms added to our *Materia Medica* from such sources are worthless, because they are not the effects of the drugs to which they are ascribed. They think that the *Sepia* had no influence in modifying the symptoms, which would have occurred if the provers had taken only Milk sugar or Alcohol under the supposition that it was a medicinal agent.

This difference of opinion suggested more complete experiments.

In 1877, two years later, Dr. Conrad Wesselhœft, chairman of this bureau, presented provings of Milk sugar. These, also, were made by medical students, and were, in every important respect, analogous to the *Sepia* provings referred to. The provers were sixteen in number, eleven male and five female.

They received from Dr. Wesselhœft packages of Sugar of milk, and took it, probably under the impression that it was a medicinal agent. At the end of six weeks, they brought forth an array of seven hundred and sixty-five symptoms, equal in se-

verity to those which had been ascribed to the attenuations of *Sepia*. The record of the symptoms is published in the *Transactions*, occupying thirty-one pages of that volume. Summarizing the above, it appears that:

Sepia 3°, Milk sugar, and fear produce, on an average, *eleven* symptoms to the prover.

Sepia 30°, Milk sugar, and fear produce, on an average, *thirteen* symptoms to the prover.

Sepia 200°, Alcohol, and fear produce, on an average, *thirty-eight* symptoms to the prover.

Milk sugar and fear alone produce, on an average, *forty-eight* symptoms to the prover.

For the sake of brevity, the term "fear" is here made to include the state of mental solicitude which so increases the sensibility of the prover as to cause him to observe conditions and sensations which ordinarily would receive no more than a passing notice. This *fear* is a potent factor in the production of symptoms in all provings like those I have mentioned. Its power varies in different individuals, and in the same individual at different times. The influence of this factor must be eliminated from the provings in order to ascertain the effects produced by the drug itself. To eliminate, we must either remove or neutralize. Since we cannot remove we must neutralize. In the third, or test method of experimentation, this end is accomplished. The prover is deprived of all bias or prejudice, in his experiments, by being kept in ignorance as to whether, in any particular instance, he is taking a medicinal preparation or a blank containing the menstruum alone. Since he is just as likely to ascribe irrelevant sensations to the blank as to the medicine, these sensations cannot influence the result. He is thus delivered from the besetting snares which impede all experimenters upon attenuated medicines, who follow the ordinary methods. A typical example of the test method is seen in a series of experiments performed in the year 1879 for the *Milwaukee Academy of Medicine*, by the believers in the medicinal power of the 30th dilutions.

The experiments were designed as a crucial test of the efficacy or inertness of the 30th dilutions.

The plan was proposed in a paper which I read before that

society in 1878. It was briefly as follows: Several vials, similar in appearance, were given to the prover. One of the vials contained the 30th centesimal attenuation of a remedy selected by the prover, while the remainder of the vials contained only a quantity of the menstruum in which the drug was prepared, namely, alcohol and cane-sugar. The prover undertook to select the medicine from among the blanks, by means of its effects upon the human organism.

The attenuations were made by the *Milwaukee Academy of Medicine* especially for the purposes of this test. The tinctures of plants and the 6th decimal attenuations of drugs, were obtained from well-known and reputable homœopathic pharmacists, and, in all cases in which the nature of the substances admitted, these original preparations were tested by members of the society to make sure of their genuineness. All of the attenuations were made at the meetings of the society, both believers and unbelievers in the 30th potency being present and witnessing the process. The vials were scrupulously clean and the Alcohol and sugar globules the purest obtainable. The attenuations were made by dilution and succussion, according to Hahnemann's directions, in the proportion of one to ninety-nine, and ten vigorous shakes were given at each dilution.

The attenuations thus prepared, together with the remainder of the Alcohol used in making them, and a sufficient quantity of labelled vials, sugar globules, and mailing boxes, were given to an unprejudiced layman of unimpeachable honor, who promised to guard them and dispense them as they were called for through the secretary of the society.

The chosen depositary was Rev. George T. Ladd, late pastor of the *Spring Street Congregational Church* in Milwaukee, now Professor of Mental Philosophy at *Bowdoin College*, Maine, and lecturer at *Andover Theological Seminary*. He is a man well known among the scholars in the church to which he belongs, and one who has borne an unblemished reputation in the communities where he has resided, and more important than all, a man who had no interests to serve in the matter except those of truth and honor. Rev. George T. Ladd took the preparations and utensils directly from the hands of the society and, himself,

mediated the pellets and dispensed to the prover the test packages. The vials were all numbered and a record was kept of their contents.

The test packages were given out at various times, as they were called for, from January 13th to July 28th, 1879. Notice was given, at the beginning, of the time allowed for the experiment, and all the provers were notified a second time when the reports were due. January 26th, 1880, Professor Ladd mailed his report to the society. As soon as practicable thereafter the society met, broke the seals and compared the report of the depositary with the reports of the experimenters.

The result is indicated in tabular form thus:

TESTS OF THE THIRTIETH CENTESIMAL DILUTION.

W. H. Blakely, Bowling Green, Ky., . . .	Aconitum, . . .	9*	Incorrect selection.
G. R. Mitchell, Richland Centre, Wis., . . .	Aconitum, . . .	9	Incorrect selection.
C. R. Muzzy, Watertown, Wis., . . .	Aconitum, . . .	9	Incorrect selection.
A. W. Woodward, Chicago, Ill., . . .	Aconitum, . . .	9	Incorrect selection.
N. A. Pennoyer, Kenosha, Wis., . . .	Aconitum, . . .	9	Incorrect selection.
M. A. Ries, Milwaukee, Wis., . . .	Aconitum, . . .	9	Incorrect selection.
A. Uhlemeyer, St. Louis, Mo., . . .	Aconitum, . . .	9	Incorrect selection.
J. H. Thompson, New York, . . .	Aconitum, . . .	9	No selection.
W. F. Morgan, Leavenworth, Kan., . . .	Aconitum, . . .	9	No selection.
J. W. Thompson, Greenfield, Mass., . . .	Aconitum, . . .	9	No report.
H. L. Waldo, West Troy, N. Y., . . .	Aconitum, . . .	9	No report.
E. Lippincott, Bowling Green, Ky., . . .	Aconitum, . . .	9	No report.
W. B. Trites, Manayunk, Pa., . . .	Aconitum, . . .	9	No report.
C. H. Hall, Madison, Wis., . . .	Aconitum, . . .	9	No report.
O. W. Smith, Union Springs, N. Y., . . .	Aconitum, . . .	9	No report.
W. Colleson, St. Louis, Mo., . . .	Aconitum, . . .	9	No report.
W. Eggert, Indianapolis, Ind., . . .	Aconitum, . . .	9	No report.
H. A. Foster, Buffalo, N. Y., . . .	Aconitum, . . .	9	No report.
T. L. Brown, Binghamton, N. Y., . . .	Aconitum, . . .	9	No report.
H. A. Foster, Buffalo, N. Y., . . .	Arsenicum, . . .	9	No report.
C. W. Mohr, Philadelphia, Pa., . . .	Belladonna, . . .	9	No report.
J. A. Pearsall, Saratoga, N. Y., . . .	Belladonna, . . .	9	No report.
E. C. Morrill, Norwalk, O., . . .	Nux vomica, . . .	9	No report.
T. M. Martin, Delavan, Wis., . . .	Nux vomica, . . .	9	No report.
N. A. Pennoyer, Kenosha, Wis., . . .	Arsenicum, . . .	4	No report.
W. M. Butler, Middletown, N. Y., . . .	Digitalis, . . .	1	No report.
C. B. Gatchell, Ann Arbor, Mich., . . .	Aurum, . . .	1	No report.
C. B. Gatchell, Ann Arbor, Mich., . . .	Arsenicum, . . .	1	No report.
C. B. Gatchell, Ann Arbor, Mich., . . .	Calcarea carb., . . .	1	No report.
C. B. Gatchell, Ann Arbor, Mich., . . .	Carbo veg., . . .	1	No report.
C. B. Gatchell, Ann Arbor, Mich., . . .	Sulphur, . . .	1	No report.
W. S. Gillett, Fox Lake, Wis., . . .	Arsenicum, . . .	1	No report.

* This column gives the number of "blanks" in each case.

W. S. Gillett, Fox Lake, Wis., . . .	Aurum, . . .	1*	No report.
W. S. Gillett, Fox Lake, Wis., . . .	Calcarea carb., . . .	1	No report.
W. S. Gillett, Fox Lake, Wis., . . .	Carbo veg., . . .	1	No report.
W. S. Gillett, Fox Lake, Wis., . . .	Sulphur, . . .	1	No report.
O. W. Smith, Union Springs, N. Y., . . .	Arsenicum, . . .	1	No report.
O. W. Smith, Union Springs, N. Y., . . .	Aurum, . . .	1	No report.
O. W. Smith, Union Springs, N. Y., . . .	Calcarea carb., . . .	1	No report.
O. W. Smith, Union Springs, N. Y., . . .	Carbo veg., . . .	1	No report.
O. W. Smith, Union Springs, N. Y., . . .	Sulphur, . . .	1	No report.
A. Uhlemeyer, St. Louis, Mo., . . .	Arsenicum, . . .	1	Correct selection.
A. Uhlemeyer, St. Louis, Mo., . . .	Aurum, . . .	1	No report.
A. Uhlemeyer, St. Louis, Mo., . . .	Calcarea carb., . . .	1	No report.
A. Uhlemeyer, St. Louis, Mo., . . .	Carbo veg., . . .	1	No report.
A. Uhlemeyer, St. Louis, Mo., . . .	Sulphur, . . .	1	No report.
W. F. Morgan, Leavenworth, Kan., . . .	Arsenicum, . . .	1	No report.
W. F. Morgan, Leavenworth, Kan., . . .	Aurum, . . .	1	No report.
W. F. Morgan, Leavenworth, Kan., . . .	Calcarea carb., . . .	1	No report.
W. F. Morgan, Leavenworth, Kan., . . .	Carbo veg., . . .	1	No report.
W. F. Morgan, Leavenworth, Kan., . . .	Sulphur, . . .	1	No report.
O. S. Childs, Beaver Dam, Wis., . . .	Arsenicum, . . .	1	No report.
O. S. Childs, Beaver Dam, Wis., . . .	Aurum, . . .	1	No report.
O. S. Childs, Beaver Dam, Wis., . . .	Calcarea carb., . . .	1	No report.
O. S. Childs, Beaver Dam, Wis., . . .	Carbo veg., . . .	1	No report.
O. S. Childs, Beaver Dam, Wis., . . .	Sulphur, . . .	1	No report.
W. Colleson, St. Louis, Mo., . . .	Arsenicum, . . .	1	No report.
W. Colleson, St. Louis, Mo., . . .	Aurum, . . .	1	No report.
W. Colleson, St. Louis, Mo., . . .	Calcarea carb., . . .	1	No report.
W. Colleson, St. Louis, Mo., . . .	Carbo veg., . . .	1	No report.
W. Colleson, St. Louis, Mo., . . .	Sulphur, . . .	1	No report.
Petrus Nelson, Minneapolis, Minn., . . .	Arsenicum, . . .	1	No report.
Petrus Nelson, Minneapolis, Minn., . . .	Aurum, . . .	1	No report.
Petrus Nelson, Minneapolis, Minn., . . .	Calcarea carb., . . .	1	No report.
Petrus Nelson, Minneapolis, Minn., . . .	Carbo veg., . . .	1	No report.
Petrus Nelson, Minneapolis, Minn., . . .	Sulphur, . . .	1	No report.
L. A. Campbell, Attleboro, Mass., . . .	Arsenicum, . . .	1	No report.
L. A. Campbell, Attleboro, Mass., . . .	Aurum, . . .	1	No report.
L. A. Campbell, Attleboro, Mass., . . .	Calcarea carb., . . .	1	No report.
L. A. Campbell, Attleboro, Mass., . . .	Carbo veg., . . .	1	No report.
L. A. Campbell, Attleboro, Mass., . . .	Sulphur, . . .	1	No report.

SUMMARY.

Ten-vial Test.

Number making reports,	9
Number making correct selection,	0
Number making no selection,	2
Number making incorrect selection,	7
Number making no report,	16
Total,	25

Five-vial Test.

Number making report,	0
Number making no report,	1
Total,	1

* This column gives the number of "blanks" in each case.

Two-vial Test.

Number making report,	1
Number making correct selection,	1
Number making no report,	46
Total,	48
Total number of tests,	73
Total number of correct selections,	1

The most remarkable feature of the result, aside from the total failure in all of the ten-vial tests which were performed, is the fact that the majority of the experimenters did not venture upon a selection. This, with the fact that comparatively few of the advocates of potentization were willing to undertake the experiment, shows a lack of confidence in potentized drugs, which was not anticipated by the proposer of the test.

If the entire body of provers of, and healers with, dynamized medicines, cannot, in the course of twelve months, distinguish one of their remedies from its inert menstruum, under conditions which preclude the aid of collusion, fraud, or chance, it may be reasonably inferred that the so-called dynamized drugs are not to be relied on as medicinal agents. There is no evidence, in the results of this test that, the 30th dilution of any medicine contains any power or property which is not also contained in the menstruum in which the medicine is prepared. The nature and conditions of the experiments were such, that if there be in these dilutions any medicinal power, which can be recognized or handled, this power would have shown its existence by its effects.

It must be borne in mind,

(1.) That the genuineness of the preparations is vouched for by competent physicians who were believers in the powers of the 30th dilutions.

(2.) That the custody and distribution of the preparations was put directly into the hands of a man of known integrity, who had no interest in the success or failure of the doctrine of dynamization.

(3.) That the experimenters were believers in the efficacy of preparations they tested.

(4.) That the experimenters chose the remedies with which they experimented and voluntarily undertook the task of selection.

(5.) That the experimenters were not limited to any special method of investigation, but were allowed full liberty to apply chemical, electrical, physiological, therapeutic, or any other tests by which they might detect a difference between the medicated and the unmedicated pills. In view of these considerations the failures to report are significant.

The advocates of dynamization have plead that the test was too hard, avowing their belief that it is impossible also to select under similar conditions the 3d, 4th, 5th, and 6th decimal attenuations.

Partly to prove the weakness of this plea, but more especially to furnish *data* for determining the limit of drug power in attenuation, a series of experiments on the lower attenuations has been performed during the past year under the auspices of this bureau.

It was arranged that Dr. T. F. Allen, my confrère in this section of the bureau, should undertake a test of the 30th dilutions, similar to that of the Milwaukee Academy, while I undertook tests of the lower attenuations on the same conditions. A committee of three were to be appointed to oversee and place safeguards around each subseries of experiments. On each committee there was to be a representative of the American Institute of Homœopathy, appointed by Dr. Dake, the chairman of the bureau; a representative of the believers in dynamization, appointed by Dr. Allen; and a representative of unbelievers in dynamization, appointed by me. The committee to oversee Dr. Allen's tests was chosen as follows:

Egbert Guernsey, M.D., of New York, by Dr. Dake; J. T. O'Conner, M.D., of New York, by Dr. Allen; C. F. Chandler, M.D., of New York, by Dr. Sherman.

Awaiting the appointment of a similar committee to oversee my tests in Milwaukee, I learned that Dr. Allen had declined to go on with his experiments, and that he would not appoint a representative in the Milwaukee committee.

Knowing well the fertility of any tests of this sort which were not satisfactory to skeptics, I had insisted upon both Dr. Allen's tests and mine being surrounded by safeguards imposed by a committee representing believers and unbelievers. The absurdity

of attempting to convince skeptics without giving them a representation in the safeguards is apparent to the wayfaring man.

After Dr. Allen's failure, I called upon the three medical societies of Milwaukee to appoint each its representative, of various opinions on the dose question, to scrutinize and place safeguards around the proposed tests. Dr. W. J. Hawkes, of Chicago, who had publicly challenged a comparative test of the 3d and 30th dilutions, was also requested to appoint a representative.

After considerable delay, occasioned mainly by the apparent unwillingness of the dynamizationists to aid in any tests, the arrangement of safeguards was completed and the experiments were begun.

The following record of the tests was made by the committee in an official manner.

RECORD OF A TEST OF THE LOW DILUTIONS.

Milwaukee, Wis., January 14th, 1880.—At 8 P.M. the following named gentlemen met in the office of Dr. Sherman on Wisconsin Street, namely :

Dr. Lewis Sherman, member of the *Bureau of Materia Medica, Pharmacy, and Provings, American Institute of Homœopathy*. Dr. C. C. Olmsted, appointed by the Chairman of the *Bureau of Materia Medica, Pharmacy, and Provings, American Institute of Homœopathy*, to represent the believers in high potencies in placing safeguards around the test.

Dr. G. C. McDermott, Dr. Samuel Potter, representing the *Milwaukee Academy of Medicine*, by special appointment.

Dr. E. F. Storke, Secretary, *Wisconsin State Homœopathic Medical Society*.

Professor George W. Peckham, teacher of biology, *Milwaukee High School*.

Absent, Dr. E. M. Rosenkrans, of the committee, appointed by the *Milwaukee Academy of Medicine*.

The committee was called to order by Dr. Sherman, Professor George W. Peckham was chosen chairman, and Dr. Samuel Potter, secretary.

Dr. Storke presented a sealed package with the following writing upon it :

"I hereby certify that the contents of this box, consisting of a piece of *Phosphorus*, one seed of *Nux vomica*, one small bottle with *Mercury*, and one with *Arsenious acid*, have been examined by me, and I found them pure and as represented; and further, that I have placed them in this box, and have wrapped them up and sealed.

"GUSTAVUS BODE,
Analytical Chemist.

"MILWAUKEE, January 13th, 1880."

The package was opened by Dr. C. C. Olmsted in the presence of the committee, and the substances enumerated above were found therein, each in a separate package. They were then placed upon a small table surrounded by the committee.

On motion, it was agreed that each member record his opinion regarding the medicinal efficacy of attenuated drugs, for the purpose of showing that all sides of the question are represented on this committee.

The chairman, Professor Peckham, stated that he had no opinion to express on the matter, not having had sufficient evidence before him to determine the question.

Dr. C. C. Olmsted stated that he believed in the medicinal efficacy of the drugs enumerated as high as the 200th decimal attenuation.

Dr. G. C. McDermott stated that he had the fullest confidence in the medicinal virtue of all drugs in the highest attenuations when attenuated by the Hahnemannian process.

Dr. Lewis Sherman stated that he had confidence in the medicinal efficacy of the drugs enumerated as high as the 6th decimal attenuation.

Drs. E. F. Storke and Samuel Potter stated that they had little or no confidence in the medicinal efficacy of the attenuation above the 6th decimal of the drugs enumerated.

Professor Peckham dissolved two grains of *Arsenious acid* with 200 minims of distilled water in a new clean test-tube.

At 9.15 P.M. Dr. Storke took his departure.

This solution of *Arsenicum* was then attenuated according to the Hahnemannian process to the 7th decimal by Drs. Olmsted

and Potter. The preparations were dispensed for proving in the following manner:

Ten two-drachm vials were labelled "*Arsenicum* 6^x," and on each label was written a different number; the numbers running from one to ten, inclusive.

Ten slips of paper were likewise numbered from one to ten. Each of the slips was carefully folded to conceal its number, and was placed alongside the cork in the correspondingly numbered vial. Each of the vials was then wrapped in a square piece of white paper (the pieces of the same size and quality), so as to leave only the cork, one end of the slip, and the rim of the vial visible. The ten vials were then placed in a hat and shaken. Dr. Olmsted then took one vial from the hat, took out the cork and the slip, filled the vial with the *Arsenicum* 6^x, replaced the cork, deposited the filled vial in another hat, marked the slip which he took out "*Arsenicum* 6^x," and deposited it unopened in a paper slide-box. The other nine vials were then filled with distilled water by members of the committee; each slip being handed without being unfolded, or the wrapper being removed from the vial, to Dr. Olmsted, who deposited it in the box with the marked slip, and sealed the box; all the members of the committee writing their names across the seals. The ten vials thus deposited in the hat were again mixed up by shaking and taken from the hat.

The set of *Arsenicum* 6^x was then, in accordance with his request, handed to Dr. G. C. McDermott for experimentation, and the whole lot of material (drugs, water, alcohol, slide-box, etc.) was placed in a desk, the same being locked and sealed, the seals being signed by Dr. Olmsted and Professor Peckham; whereupon the committee adjourned to meet at the call of Dr. Lewis Sherman.

January 22d, 1880, 8.30 P.M. The committee met at Dr. Sherman's office. Present: Professor George W. Peckham, Chairman, Dr. Samuel Potter, Secretary, Drs. E. M. Rosenkrans (of the committee of the Milwaukee Academy of Medicine), C. C. Olmsted, and Lewis Sherman.

Dr. G. C. McDermott, having been called to the chair of Oph-

thalmology and Otology at Pulte Medical College, had left the city.

Dr. Sherman dissolved four (4) grains of Phosphorus in a boiling flask in 2800 minims of Alcohol, in the view of the committee.

A package of Belladonna tincture, bearing the original seal of a well-known European pharmacist, was opened by the committee, and a portion thereof was attenuated to the 7th decimal dilution by Drs. Olmsted and Rosenkrans. A set each of Belladonna 3^x and 7^x were then dispensed by Drs. Olmsted and Potter in the manner heretofore described, the boxes of slips sealed, and the packages handed to Dr. Lewis Sherman for experimentation. The material was then placed in the sealed desk, as heretofore described, and the committee adjourned.

February 26th, 1880, 8.30 P.M. The committee met. Present: Drs. Rosenkrans, Olmsted, Sherman, and Potter. Professor George W. Peckham having promised to be present, but failed. The desk-seals were examined. One was found intact, the others having burst in drying. Arsenicum 5^x, 6^x, and 7^x were then dispensed by the committee in the manner heretofore described, Dr. Olmsted doing the medicating, marking, filling, and sealing of the boxes containing the slips. They were then handed for experimentation as follows:

Ars. 5^x and 6^x to Dr. Lewis Sherman.

Ars. 7^x to Dr. Samuel Potter.

After placing the material under seal in the desk, the committee adjourned.

March 16th, 1880, 8.50 P.M. The committee met. Present: Drs. Sherman, Olmsted, Rosenkrans, and Potter. Aconitum 3^x and 6^x were prepared from the tincture found in stock in Dr. Sherman's pharmacy. Phosphorus 5^x and 6^x were made from the solution prepared by the committee and kept under seal. They were then dispensed as follows, in the manner heretofore described: the filling, marking, and sealing being done by Dr. C. C. Olmsted in person, or under his immediate direction, by different members of the committee. To Dr. Potter were handed

sets of Aconitum 3^x and 6^x, Phosphorus 5^x and 6^x. To Dr. Sherman sets of Aconitum 3^x and 6^x, Phosphorus 5^x and 6^x.

The material being placed under seal as before, the committee adjourned.

March 25th, 1880, at 8.30 P.M. The committee met. There were present Drs. Olmsted, Rosenkrans, Sherman, and Potter. Dr. Olmsted received the reports of Drs. Sherman and Potter. The boxes containing the marked slips were then examined by the committee, and with their seals were found intact. They were then opened by Dr. Olmsted, and the marked slips found to agree with the reports as follows :

Preparation tested.	NUMBER OF THE MEDICATED VIAL.		
	Dr. Potter's report.	Dr. Sherman's report.	Record in sealed box.
Aconitum, 3 ^x	1	1	1
Aconitum, 6 ^x	1	1	1
Arsenicum, 5 ^x		6	6
Arsenicum, 6 ^x		9	9
Arsenicum, 7 ^x	6		6
Belladonna, 3 ^x (1).....		1	8
Belladonna, 7 ^x (1).....		2	9
Phosphorus, 5 ^x	1	1	1
Phosphorus, 6 ^x	7	7	7

The committee then adjourned.

March 30th, 1880. The committee met at Dr. Sherman's office. There were present Dr. Lewis Sherman, Dr. C. C. Olmsted, Dr. E. M. Rosenkraus, and Dr. Samuel Potter.

The minutes of the previous meetings were read and approved.

Belladonna 3^x and 6^x, Phosphorus 8^x, 9^x, and 10^x were then prepared as before, from the tinctures—the Belladonna from a new tincture—and were dispensed by Dr. C. C. Olmsted in ten-vial sets, as heretofore described. They were handed out as follows: To Dr. Potter, Belladonna 6^x, Phosphorus 9^x, and to Dr. Sherman, Belladonna 3^x, and Phosphorus 8^x and 10^x.

After sealing the sealed boxes in the desk drawer heretofore used, the committee adjourned.

April 25th, 1880. The committee met. There were present Drs. Olmsted, Rosenkrans, Sherman, and Potter.

Dr. Olmsted received the reports of Drs. Sherman and Potter, who also stated that they had each, before experimentation, divided with each other the medicines given to them by the committee. The boxes containing the marked slips were then examined by the committee, and with their seals were found intact. They were then opened by Dr. Olmsted, and the marked slips found to agree with the reports received, as follows :

Preparation.	NUMBER OF THE MEDICATED VIAL.			
	Dr. Potter's report.	Dr. Sherman's report.	Record in sealed box.	
Belladonna, 3x (2).....	2	2	2	
Belladonna, 6x (2).....	9	1	9	
Phosphorus, 8x.....	5	7	7	
Phosphorus, 9x.....	5	2	5	
Phosphorus, 10x.....	4	10	1	

SUMMARY.

Both experimenters were successful with Acon. 3x, Acon. 6x, Arsen. 5x, Arsen. 6x, Arsen. 7x, Bell. 3x, No. 2, Phos. 5x and Phos. 6x.

Both experimenters failed with Phos. 10x.

One experimenter succeeded and one failed with Bell. 6x, No. 2, Phos. 8x, and Phos. 9x.

One experimenter failed with Bell. 3x, No. 1, and Bell. 7x, No. 1.

The committee then adjourned.

GEORGE W. PECKHAM,
Chairman.

C. C. OLMSTED, M.D.,
EUGENE F. STORK, M.D.,
LEWIS SHERMAN, A.M., M.D.,
E. M. ROSENKRANS, M.D.,
SAMUEL POTTER, M.D.,
Secretary.

For convenient reference I have arranged the preceding reports so as to make them correspond with that on the 30th dilution, by the Milwaukee Academy of Medicine.

TESTS OF THIRD DECIMAL DILUTION.

Samuel Potter,	Aconitum,	9*	Correct selection.
Lewis Sherman,	Aconitum,	9	Correct selection.
Lewis Sherman,	Belladonna No. 1, . .	9	Incorrect selection.
Samuel Potter,	Belladonna No. 2, . .	9	Correct selection.
Lewis Sherman,	Belladonna No. 2, . .	9	Correct selection.

TESTS OF THE FIFTH DECIMAL DILUTION.

Lewis Sherman,	Arsenicum,	9	Correct selection.
Samuel Potter,	Phosphorus,	9	Correct selection.
Lewis Sherman,	Phosphorus,	9	Correct selection.

TESTS OF THE SIXTH DECIMAL DILUTION.

Samuel Potter,	Aconitum,	9	Correct selection.
Lewis Sherman,	Aconitum,	9	Correct selection.
Lewis Sherman,	Arsenicum,	9	Correct selection.
Samuel Potter,	Phosphorus,	9	Correct selection.
Lewis Sherman,	Phosphorus,	9	Correct selection.
Samuel Potter,	Belladonna No. 2, . .	9	Correct selection.
Lewis Sherman,	Belladonna No. 2, . .	9	Incorrect selection.
G. C. McDermott,	Arsenicum,	9	Lost the package.

TESTS OF THE SEVENTH DECIMAL DILUTION.

Samuel Potter,	Arsenicum,	9	Correct selection.
Lewis Sherman,	Belladonna No. 1, . .	9	Incorrect selection.

TESTS OF THE EIGHTH DECIMAL DILUTION.

Samuel Potter,	Phosphorus,	9	Incorrect selection.
Lewis Sherman,	Phosphorus,	9	Correct selection.

TESTS OF THE NINTH DECIMAL DILUTION.

Samuel Potter,	Phosphorus,	9	Correct selection.
Lewis Sherman,	Phosphorus,	9	Incorrect selection.

TESTS OF THE TENTH DECIMAL DILUTION.

Samuel Potter,	Phosphorus,	9	Incorrect selection.
Lewis Sherman,	Phosphorus,	9	Incorrect selection.

With a single exception there were no failures to report in the low dilution tests. The following note from Dr. McDermott to the Secretary of the Committee explains the absence of his name in the table :

CINCINNATI, OHIO, April 30th, 1880.

DEAR DOCTOR POTTER : In reply to yours of inquiry regarding the test, I am sorry not to be able to report even trying to find the medicated preparation. If you remember, it was just two days before I took my departure, and the medicines were mislaid, as I was not there during the moving. I regret sincerely the accident.

Ever yours,

G. C. McDERMOTT.

* This column gives the number of "blanks" in each case.

*THE PROOFS OF DRUG PRESENCE AND
POWER IN ATTENUATIONS ABOVE
THE SIXTH DECIMAL,*

AS FURNISHED BY ANALOGY FROM THE FIELD OF IMPAL-
PABLE MORBIFIC AGENCIES.

BY J. P. DAKE, M.D., NASHVILLE.

IF a drop of blood, or a particle of pus, or a breath of air, in which chemical tests and the spectroscope and the microscope can detect no impurity, is found, by human experience, to hold the seeds of disease, analogy warrants the conclusion that, in drug attenuations where such tests and appliances fail to discover a drug atom, there yet may be medicinal presence and power.

The field of morbid agencies is very broad, and, for the most part, covered with thick darkness. Human vision has discerned much in it by occasional gleams of light in efforts to learn the origin and to trace the history of various diseases.

Inheritance.—In the ovum and the seminal fluid are borne, from parent to offspring, those material yet impalpable seeds which, germinating in after years, give rise to *tuberculosis*, *carcinosis*, the *hydra-headed syphilis*, and other affections which are regarded as hereditary. No species of test, no means of diagnosis have yet been able to detect those seeds in advance of their germination and fruitage; and yet, where is the pathologist who ventures to deny their existence?

Parasites.—In articles of food and drink, in clothing, in beds, and in contact with persons diseased, various germs are received, which increase or develop into parasites—*epizoa*, *entozoa*—to prey upon and even destroy the human body. Some of these germs have been clearly seen and studied by direct observation,

while others have eluded the keenest search with means the most perfect and powerful.

Judging the unseen by that which is seen ætiology has come to acknowledge the presence and sick-making power of living germs which no microscope has discerned.

Vegetable Emanations.—There are trees and plants the emanations from which occasion sickness and even death. There are persons who cannot ride along a road, beside which grows the *Rhus toxicodendron*, without having vesicular erysipelas, especially if the wind chance to blow from the direction of that shrub. The senses all fail to detect the poison thus borne upon the air. It is impalpable, yet who denies its potency?

Miasmata.—There are localities where one night's sojourn, where one hour's travel, may occasion intermittent fever, malarial fever, and other affections which we denominate miasmatic.

Whatever may be our views of the pathology of such affections—whatever organ or tissue we may regard as the one primarily or chiefly affected—we cannot escape the conclusion that in such localities there is a *material* not elsewhere found, a morbid something as real as that from the poison oak, the leprous hand, the trichinous pork, or the syphilitic parent.

Efforts have been made to discover the form and other physical characteristics of miasmatic poison by the aid of the microscope and chemical reagents. Some investigators claim the ability to detect and define them in the atmosphere of various localities.

Contagion.—A healthy child entering a house where one is sick with scarlet fever, encountering nothing in the least offensive to the senses, in a few days has scarlatina.

Another, looking through a picture-book lately handled by a child recovering from measles, in due time presents a case of measles.

And another, riding for half an hour in a railway coach with a child that has mumps or whooping-cough, noticing nothing disagreeable, in a few days, perhaps a thousand miles away, is found to have mumps or whooping-cough.

A person never vaccinated nor inoculated with variolous matter, riding along a country road, on a frosty morning, a mile to the windward of a house in which is a case of small-pox, in two

weeks thereafter, without further exposure, may have that dreaded disease.

In all these cases there is a *contagium*, a graft-germ, a material, passing from person to person, which no microscope has been able to define and no chemical tests to distinguish. Each seed has its own form and character, always reproducing its kind, without material variation. Hybridization may give rise to new species, and so extend the number of impalpable morbid organic agencies.

Gases.—Beside the transmissible seeds of disease, coming by inheritance, the invisible graft-germs and parasites, and the yet more impalpable miasms and contagions from various sources, there are gases in peculiar states and proportions, inorganic elements, immediately recognizable by no means or methods at our command, which operate as effective causes of disease.

Electricity.—And beyond these is the subtile agent known as electricity, appreciable only under peculiar circumstances, yet brought under human control and measurement. In dealing with it we are compelled to consider its *quantum* as well as *quo modo*. With all its tenuity its sick-making power is not questioned.

But we need look no farther in the field of impalpable morbid agencies.

Turning now to the data furnished by chemical tests and by spectroscopic and microscopic examinations of attenuated drugs, we find medicinal matter traced beyond the 6th decimal to the 8th, 9th, and some claim to the 12th.

One mathematician, upon the accepted theory of molecular magnitudes, tells us that one grain of the 11th centesimal attenuation may contain one atom of the drug-matter and not more; while another says the 15th centesimal dilution has one atom to each drop, and the 16th but one atom to every one hundred drops.

Taking the decimal scale the calculations of the former would warrant us in the conclusion that no medicinal matter exists in the 25th attenuation, or at least but one atom in every million of drops, while the calculations of the latter, considering the ultimate atom, or indivisible particle to be reached in the

15th centesimal, would lead us to expect one atom of drug in each drop of the 30th decimal.

Beyond the 30th, so far as I am informed, no one claims either the practical or the theoretical presence of drug material.

As for drug-power existing beyond drug-presence I fail to find any proofs of such a condition in the field of morbid agencies. If the physiologist finds undoubted drug-effects from attenuations in which neither direct observation nor analogy can consent to there being drug-presence, we are only forced to the conclusion that the material drug may exist beyond the limits set by direct observation and analogy.

However we may theorize and philosophize about the persistence of force and its correlation, we cannot predicate potential drug-force where there is not some drug-matter. The force of a drug, its spirit, if you please, is no more recognizable and useful out of its own body than is the spirit of a man out of its body among men. If it were possible for it to be transferred by trituration or succussion to sugar of milk or alcohol, what would be the gain? Why labor so long to free the medicinal spirit from one body to have it "clothed upon" by another body? To those who recognize a dynamic medicinal spirit in the most inert substances, including sugar of milk, the question is pertinent: When the drug-spirit has taken possession of the sugar of milk, where is the dwelling-place of the disembodied spirit of the sugar of milk?

Let us consider these and other troublesome questions before we declare our unqualified adherence to the doctrine of medical spiritism. In avoiding the grossness of the old school let us not rush into the absurd transcendentalism of a small section of the new.

Tracing medicinal matter by one means and another, step by step, from the crude up to the finest, let us endeavor to so treat and so employ it as to escape the evil and obtain the good in the restoration of human health.

PROOFS OF MEDICINAL POWER IN ATTENUATIONS ABOVE THE SIXTH DECIMAL.

BY C. H. LAWTON, M.D., WILMINGTON, DEL.

I HAVE the honor to present for your consideration a few thoughts with reference to the limits of drug attenuation, or proofs of medicinal power in attenuations above the 6th decimal. In doing so I shall not follow the beaten track, but shall call your attention to laws and principles that control all material substances, and shall endeavor to trace some analogy between them and the action of our homœopathic remedies.

Viewing the subject from this standpoint, I am aware, I go on dangerous ground; for this question has been the cause of more dissensions in our school, and has provoked more violent opposition, than all other questions combined.

Standing, as I do, in the presence of men who are veterans in the homœopathic ranks, men of culture and ability, and at whose feet I love to sit, it is natural I should feel some embarrassment in presenting a subject of such vital importance. With no wish to provoke a spirit of controversy, but with an earnest, honest desire, to search after the truth, I ask that you join me for a few moments in the investigation of this subject, the truth of which can only be approximated by the closest observation, and oft-repeated experiment. I say approximated, for we cannot understand it by experiment; it is still shrouded in mystery.

Gentlemen come here, year after year, and relate their experience, piling fact upon fact, showing the wonderful results achieved by the most minute doses; and year after year, others

listen incredulous, and some are almost ready to give the lie, to what they cannot understand. Why is this?

One great cause, it seems to me, lies in the fact, that we are too much in the habit of using the terms high attenuations, high dilutions, and high potencies, indiscriminately. If, in making a statement, I use the terms arbitrarily, and not in harmony with the facts I wish to communicate, no logical mind can accept it. If I speak of increased medicinal power in connection with high attenuations and high dilutions, there is an incompatibility, a want of harmony in these terms, and its acceptance by others must depend upon its statement in terms that are in harmony with known laws—all truths must harmonize! Where then is the truth?

Again, gentlemen cannot accept facts based solely on experiment; they must either make the experiment themselves, or we must appeal to their judgment and reason; they must not only have facts, but these facts must harmonize with known laws. If in experimenting we develop facts *only*, our sphere of usefulness is limited; but if in developing facts, we unfold the underlying principles, while acting consistently with those principles, we can never make a mistake. It is a fact that the high potencies do act, although we cannot all accept it. Some of us know this by experience; others know it from the testimony of men standing in the foremost ranks of homœopathy, men of profound research and rare mental attainments, members of our own American Institute, and the peers of any who have lived since the days of Hahnemann.

We have the facts, either by experience, or from testimony; but science does not consist in an accumulation of facts merely, there must be a knowledge of principles whereby we may trace the *unknown*, from known phenomena, and may arrive at definite conclusions. Now, if we find that these facts harmonize with known laws, we have demonstrated a principle, and may draw conclusions which any logical mind can accept. In the further development of this subject I wish to submit the following propositions:

1st. The practical limit of drug attenuation is determined by the amount of resistance (inherent in the drug) opposed to the disorganizing or decomposing agent with which it is triturated;

and this resistance depends on density and hardness. It is, therefore, impossible to establish any arbitrary rules for defining it.

2d. That beyond the limit of drug attenuation there is medicinal power.

Some minds are so organized that they can only accept as evidence that which is appreciable to the senses ; if a thing looks strong, and smells strong, and tastes strong ; if they can handle it, and it feels strong, they conclude that it is strong. We *know*, however, that the most powerful agents of which we have *any* knowledge, are those which we cannot see, nor smell, nor taste, nor handle. Electricity and magnetism are such. There is in every material substance (therefore in every drug substance) a force, which is the life of that substance, the organizing and controlling power, the active principle.

When any substance, solid or fluid, is decomposed, this latent force is set free, and becomes active or dynamic. This principle may be evolved either chemically or by friction, heat or magnetism. All substances continue in a state of rest unless acted upon by some external force. These are fundamental principles in science and philosophy, that no argument of mine can strengthen, and no amount of opposition can disprove. I will give a few illustrations.

Take any two substances, one of which is more easily oxidized than the other, bring them in contact with a third substance, which will act upon them chemically, and as the substance most easily acted upon is decomposed, there is a principle evolved which we can neither see, nor smell, nor taste, nor handle, but which, nevertheless, is the life principle, and was the organizing and controlling power of the substance acted upon.

This is beautifully illustrated by the galvanic battery, the life principle evolved by which is called chemical or galvanic electricity. This principle may also be developed by friction ; the experiments with the glass rod, and with sealing wax, are so familiar, that it will be hardly necessary to do more than allude to them. In this case the principle developed is called frictional electricity. Heat will also develop this principle ; take two bars of different kinds of metal, bring the ends into close contact, and heat them at their junction, and a like phenomenon will occur.

Now, gentlemen, my thought is, that just as electricity dwells in all substances, and is made sensible only by friction, or decomposition, so in our potentized remedies the medicinal power is developed by friction or trituration. We know by experience that there is power developed by trituration. Is not this suggestion then, of likeness to electricity or magnetism, a suggestion in the right direction? This has been called life, without sense. I believe it is the life principle, and in the hands of Deity, the creative and creating power of the universe. Again, matter can neither give itself motion nor deprive itself of it. My hands have no power in themselves to move; my blood has no power in itself to circulate, my heart to throb, or my lungs to perform the function of respiration. They must be acted upon by some force external to themselves in order to perform the various kinds of motion to which they are adapted.

One more thought and I will close. It has been stated, and experience proves its truth, that, in the majority of cases, both acute and chronic, our potentized medicines act more promptly than the tinctures or low attenuations; I will go further, and think I may be justified in saying, that without potentization there is, and can be, no medicinal power. As material substances have no power in themselves to act, and as there can be no force without decomposition; I submit, that, if we give our medicines in the crude state, or in the low attenuations, this process of decomposition must be performed in the stomach before the medicinal power can be developed. Hence the retarded action of low attenuations.

In conclusion, I submit the following:

1st. That attenuation is simply the commingling, or the uniting in one mass, of a medicinal with a non-medicinal substance.

2d. To potentize, or in other words, to develop medicinal powers there must be decomposition.

3d. The amount of medicinal power is in proportion to the complete or partial decomposition of the drug acted upon.

4th. A point, when decomposition is complete and the medicinal power fully developed, marks the limit of drug attenuation.

*THE PROOFS OF MEDICINAL PRESENCE
AND EFFICACY IN ATTENUATIONS
ABOVE THE SIXTH DECIMAL,*

AS FURNISHED BY CLINICAL EXPERIENCE IN THE USE
OF ATTENUATIONS RANGING FROM THE FIF-
TEENTH TO THE THIRTIETH DECIMAL.

BY A. C. COWPERTHWAIT, M.D., IOWA CITY, IOWA.

MANY centuries ago it was decreed that man should not live by bread alone, or, in other words, that life was not altogether made up of those things which constitute the material, but that it must draw much of its pabulum from the fountains of the unknown. So, in all the elements which go to make up life, from its humblest to its most exalted manifestations, from the child in the cradle to the gray-haired scientist, do we find all the relations of life involving both the known and the unknown quantities. Were it not so, advancement, both moral and intellectual, would cease, for without the incentive of the unknown continually before us, the desire to unravel its mysteries would not exist, inductive reasoning would become unnecessary, and man, contented with himself and his surroundings, would cease to be the active, inspiring element of progress which he now is, and would become a mere listless automaton, performing mechanically the necessary duties of life.

Such, comparatively, is the man of the present day who attempts to deal only with known quantities, who believes only that which the sense of touch or sight may reveal to him. That physician who refuses to traverse that borderland in which dwells the unknowable of the scientist is in serious danger of shipwreck

on the shoals of inactivity. For that which is unknowable to-day may not be unknowable to-morrow, and the drug potency yielding no medicinal molecules to-day may, in the hands of the future scientist, become rich in the finely divided elements of the original substance.

Hahnemann (when he discovered that under the guidance of the law of cure the sensitive human organism would not tolerate as large doses as he had previously employed) sought to establish some plan by which the size of the dose might be systematically reduced. Of the success of his efforts we are well aware. When the aggravation of symptoms still attended the administration of the lower preparations, and he felt the necessity of still further reducing the size of the dose, Hahnemann, although appreciating his far departure from the original substance, did not point the microscope to each successive potency in order to assure himself of actual drug presence. This he considered an unnecessary test, for equally conclusive to him was the action of the medicine upon the diseased organism. When, as he ascended the scale of potencies, he saw precisely the same effects attending the administration of his remedy, save the absence of the medicinal aggravation, he wisely concluded that such action could only result from the drug administered, else why could these results be foretold with such precision? The patient tossing about with a burning fever on receiving Acon.³⁰ would break out in a profuse perspiration and the fever would abate. And this, occurring time and time again, was conclusive evidence to Hahnemann that Aconite did it. Should it not be conclusive evidence to others as well? And when Hahnemann's experience has been repeated many thousand times by his followers, not only with Aconite as mentioned, but also with many other remedies, should not the medicinal efficacy existing in the 30th potencies be clearly acknowledged, and the subject no longer remain an open question? It is a fact acknowledged by all homœopathists that the curative action of a drug, when properly selected, is not in direct ratio with the quantity of the drug employed. This principle was set forth when Hahnemann passed from crude materials, by the scruple and drachm, to drops and grains of tinctures and triturations, just as much as when urged by additional experi-

ence he passed from the latter to pellets of the 30th potency. The physician who prescribes only low potencies admits the truth of this statement equally with him who relies upon the higher potencies. So, we may safely assume it to be an accepted doctrine of the homœopathic school that, "the curative power of drugs is not in the proportion of their material quantity." This fact once acknowledged, where shall the line be drawn, and who shall say at what point in drug potentizations medicinal efficacy is lost? Again, it is a conceded point that trituration and dilution develop curative power in substances which in their crude state are inert, viz., silica, salt, tin, gold, etc. If this be the case, and it most certainly is, who is to say what point marks the limit of this development? Experience alone can establish these points, and it has already demonstrated beyond cavil that, to say the least, development of curative power does not cease below the 30th potency. I say experience has demonstrated this fact. If not, then experience is of no value, and the accumulated wisdom of Hahnemann, of Bonninghausen, of Dunham, and others only fills a blank page in our literature.

Statistics go far toward proving the medicinal efficacy of the 30th potencies, but I will detain you only by referring to the well-known experiments of Drs. Wurmb and Kasper, in the Leopoldstadt Hospital at Vienna. These gentlemen had no confidence whatever in high potencies, but determined to establish a series of experiments in order to arrive at a solution of the question of dose. So during the ten years from 1850 to 1859, inclusive, all cases in this hospital were treated for the first three years with the 30th decimal dilution, for the second three years with the 6th, and for the remaining four years with the 15th. At the end of the ten years Dr. Eidherr undertook the task of collating the material thus obtained, and confined his investigations to a single disease, pneumonia, for the reason that the diagnosis of this disease is easy, and that, by means of the physical signs, its course, progress, and decline could be more accurately followed and observed than is the case with many other acute diseases. His investigations were conducted with the greatest care, and all outside influences taken into consideration, even to the different atmospheric conditions

of each period. As regards the latter, it was determined that "during the first epoch the atmospheric conditions were most favorable to the prevalence and severity of pneumonia, and therefore the *least* favorable for the treatment; during the *second* epoch *least* favorable to the spread and severity of pneumonia, and therefore the *most* favorable for the treatment."

The cases occurring during the first period were designated by Dr. Eidherr as Group 1, those during the second period as Group 2, and those of the third period as Group 3.

The physical signs of each stage of the disease were very carefully noted, and the comparison between the groups made with reference to all these points. The following is the result:

The average duration of the infiltration was:

For Group 1,	3.0 days.
" " 2,	4.1 "
" " 3,	3.0 "

Resolution began:

For Group 1,	on the 3d day.
" " 2,	" 3 5 "
" " 3,	" 3.2 "

Resolution was complete:

For Group 1,	on the 4 9 day.
" " 2,	" 6.9 "
" " 3,	" 6.3 "

The physical signs of the infiltration vanished:

For Group 1,	on the 7 1 day.
" " 2,	" 9.3 "
" " 3,	" 10 3 "

The physical signs of the exudation vanished:

For Group 1,	on the 12.3 day.
" " 2,	" 20 5 "
" " 3,	" 18.1 "

The average duration of each case from its reception to its dismissal:

For Group 1,	11.3 days.
“ “ 2,	19.5 “
“ “ 3,	14.6 “

From these valuable figures we not only learn positively that there is medicinal power in the 30th potency, but we may safely conclude, in fact must conclude, that 30th potencies, at least in pneumonia, are to be preferred to the lower potencies.

In addition to these statistics we should not fail to take account of the success of those individual practitioners who have relied mostly upon the 30th, or still higher potencies—Hahnemann, Bonninghausen, and others, in both Europe and America. These gentlemen have won world-wide reputations as successful practitioners, and, to say the least, their successes in practice will very favorably compare with those of our friends who rely exclusively upon the low preparations. Nor should we forget a fact in the history of homœopathy, recently brought to notice by Dr. Lippe, who writes as follows: “Homœopathy was introduced into the United States about fifty years ago. The early pioneers, as we well remember, had only the 30th potency at their command. The only shop in the United States where homœopathic medicines were for sale was in Broad street, Philadelphia, kept by J. G. Wesselhœft, and this bookseller imported them from Europe. With these 30th potencies as their only outfit in the way of medicine, but with a full knowledge of Hahnemann’s teachings, and strictly following the rules laid down in the *Organon* in the application of the principles governing the homœopathic healing art, did these early pioneers enter upon their mission ‘to cure the sick.’ Our skeptics of the present day, then, should remember that the foundation upon which homœopathy has been so rapidly and so successfully builded in America, was laid by the successful use of the 30th potencies. Had the experiment, though such it was not considered, failed at that time, had it been proven that the 30th potencies contained no curative power, homœopathy would to-day have been a thing of the past. Had it not been for the triumphant successes which attended the use of these potencies in the hands of those pioneers, the young members of our profession to-day, instead of attempting to unsettle facts most thoroughly established fifty years ago,

would have been entirely ignorant, save from history, that such a school of medicine ever existed. How ungrateful, then, and foolish, to assail their own birthright, and to attempt to tear down the foundation upon which they themselves are builded, and to which they owe their professional existence."

But the materialists of the present day would have us understand that clinical proofs are of no validity, and very wisely turn to the patent medicine almanac as proof of their position. To admit such a comparison should hardly be allowed. The homœopathic remedy prescribed upon a scientific basis is expected, according to the law of similars, to effect certain definite results, which may be presaged with remarkable certainty. On the other hand, the so-called clinical proofs of the patent medicine action are only the indefinite and unwarrantable conclusions of unprofessional minds, enlarged and embellished by the proprietor of the nostrum for pecuniary benefit. There is no similarity whatever between the two.

That clinical proofs, when not surrounded by proper restrictions, and when not conducted by observers of integrity and scientific capacity, may become worthless we admit, but it is not of such that we would speak, nor is it necessary to take such into account when we have before us the authentic and reliable case-books of Hahnemann, Bonninghausen, Dunham, and many others. So when we have clinical proofs which are the result of the single homœopathic remedy prescribed in precise accordance with the homœopathic law of cure, and untrammelled by any deleterious or antidotal influences, and in cases where the diagnosis is established beyond question, may we safely say that such proofs are of equal importance and rank with those proofs which, to the chemist, decides the proportions of a compound, or, to the physiologist, establishes the relations of the life processes, or which tells the astronomer in advance the movement of the heavenly bodies. Each is the definite result of an applied natural law, and in this sense each is of equal importance to the other.

Not only is there therapeutic power in the 30th potency, but pathogenetic power as well. Hundreds of authentic provings with the 30th potencies are recorded, in which the positive and

distinct pathogenesis of the drug, as afforded by poisonings and by provings with the crude material, are verified. If then the 30th potencies have been proved to possess pathogenetic power, their therapeutic virtues must be acknowledged. Nor can we overcome the facts of these authentic provings by sneering at motives, or by calling into question the veracity or reliability of the prover. Take, if you please, the provings of *Rhus rad.*³⁰ made by Dr. Joslin. Note the rheumatic character of the pains produced, even to the retaining of the *Rhus* peculiarity, worse when first beginning to move. Also observe the skin symptoms giving the peculiar eruption with much burning and smarting, so that Dr. Joslin himself remarks that, "the effects apparently produced by the 30th, agreed in character and location very closely with those caused in many by the contact of the legs and feet with the leaves of the living vine." Can it be said that such symptoms were the result of imagination? Not at all. This, 'tis true, only argues as to the pathogenetic power, but we need not go far to establish the curative. My own experience, aside from that of scores of others present, guarantees the fact that this class of symptoms have very many times been removed by *Rhus*³⁰, nor is it necessary to prove a fact so positively well known. Yet in this connection pardon a single case: Dr. Hupfield reports the case of a colored woman aged 33, who had had rheumatism for five years, suffering every day, and at times very severe attacks. The chief characteristic being "excessive pain and stiffness of the limbs," the latter especially on rising after being seated. Four doses of *Rhus tox.*³⁰ produced a radical cure. A few special cases, and I will close:

Dr. Fanning reports the following: A woman, aged 28, after an attack of typhoid fever under old-school treatment, had a chronic diarrhoea alternating with constipation lasting over a year; the chief peculiarity of the diarrhoeic attacks was a sensation of something pulling at the umbilicus, with actual retraction at the navel. A few doses of *Plumb. met.*³⁰ gave an immediate cure.

Dr. Brown, of Binghamton, reports the case of a lady having paroxysms of excruciating pain at the stomach, as if "sticking the parts with a knife," which had lasted under allopathic

and homœopathic treatment over a year. One dose of Phos.³⁰ cured immediately.

Dr. Betts reports a case of constipation, with no desire for stool or ability to pass stool, cured with one dose of Alumina³⁰.

Dr. Boparius reports a case of flatulence of several years' standing cured with Phos.³⁰

My colleague on this bureau, Dr. Lawton, of Wilmington, Del., furnishes the following remarkable case: A lady, subject to periodical attacks of renal colic, which had always yielded promptly to Lyc.³⁰ (Tafel), sent for medicine saying, "You know just what I want, doctor; it is one of my old attacks." I sent her one powder of Lyc.³⁰ to be dissolved and taken in water, a teaspoonful every half hour until relieved. I also sent a second powder (Sac. lac.) to be dissolved and taken after the first had been discontinued. In about three hours I was sent for to come immediately, the patient being a great deal worse. Found her symptoms all aggravated, while she was tossing from one side of the bed to the other in great agony.

I asked if she had been better at any time since taking the medicine, and was told that after taking *two* doses she was relieved and thought she could sleep. In the meantime her medicine became again due, and she took another dose. From that time she became uneasy, and continued to grow worse every time the medicine was repeated. I decided that she was getting aggravations from Lyc., and stepping to the bed gave a few pellets of Camph.³⁰ dry on the tongue. In less than a minute she was relieved and had no return of the pain.

From my own case-book a few cases:

Three cases of facial paralysis,—two of long standing, and one recent,—each cured within ten days, the recent case in three days, with Caust.³⁰

In a low case of typhoid fever, fifteenth day. Patient unconscious, trembling of lower lip, constant for five days. One dose Arnica³⁰; in half an hour the trembling ceased and did not return, and on further use of the same remedy consciousness soon returned and the patient made a rapid recovery.

I had always been afraid to rely upon the higher potencies in the treatment of croup, until upon one occasion I found my-

self several miles in the country with my Aconite vial empty. The 30th was all I had, so I prescribed it, and instead of waiting half an hour or longer, as I had previously done, for the much-desired perspiration and accompanying relief, I only had to wait five minutes, and I saw the most rapid and beautiful results I had ever witnessed. To this day Acon.³⁰ in such cases has never failed me.

In my college clinic, a case of constipation in a scrofulous young man of 22, a cripple from white swelling, who had not had a natural evacuation since five years of age, either using injections or cathartics, was cured in three months with Bry.³⁰ on the indication, "stool hard and dry, as if burnt."

Also a case of intermittent of six months' duration under old-school treatment. Shaking every day or every second day, at irregular intervals; chills and heat mixed; pale, yellow, cachectic look. Prescribed Ars.³⁰. Not another chill occurred, and the patient recovered at once.

But these are only a few cases out of thousands that might be offered in connection with other arguments we have adduced to show the "medicinal presence and efficacy in the 30th potency as furnished by clinical experience." Why need we go farther? These are incontrovertible facts, and, surely, if they do not convince, you "would not be persuaded though one rose from the dead."

*PROOFS OF MEDICINAL PRESENCE AND
EFFICACY IN ATTENUATIONS ABOVE
THE THIRTIETH DECIMAL,*

AS FURNISHED BY THE TESTS OF CLINICAL EXPERIENCE.

BY C. H. LAWTON, M.D., WILMINGTON, DEL.

THE strongest evidence and the most convincing proof of medicinal presence and curative power in our high potencies is that they cure. Having been assigned by our chairman the work of collecting proofs from clinical experience of medicinal power in the use of attenuations above the 30th decimal, and believing that the testimony of a number of physicians from different parts of the country would be more convincing and conclusive than the limited experience of any one man, I have solicited and obtained the following reports from gentlemen who are well known to the most of you as close observers, successful practitioners, *able exponents* of the homœopathic law, and worthy the confidence and support of every member of this Institute.

These reports, so far as time will permit, I now have the honor to read.

Mr. S. K., age 44, married, by occupation a merchant. Active habits; bathes twice a week; five feet ten inches in height; gaining in flesh; complexion sallow. Has been ill a year and a half. Father was subject to rheumatism of legs, rhus-type. Former treatment allopathic. Has considered himself incurable, and has had no treatment for several months. His temperament is nervo-bilious-sanguine, is very nervous and irritable; pulse very weak, and 66 to the minute. If startled or frightened has palpitation

of the heart; sounds of the heart normal. Circulation in hands and feet good, except in the winter. Tongue slightly coated; teeth sound; appetite good; sometimes whilst eating, nausea, and sometimes vomits ingesta. Stools undigested; bowels constipated. Rheumatic pains in right shoulder, caused by draft of air, followed by an attack one year ago. Has stiffness of thighs without pain, better from constant motion. Feels weak, and is unable to bear fatigue. Intense itching of the skin, with formation all over the body, without any eruption; has no periodicity, except it is *worse while undressing*. On this date, October 31st, 1874, I gave him one dose on the tongue of Oleander²⁰⁰, and followed with a dose, every three hours, of the same.

November 4th, 1874. Pulse 76. Tongue not changed in appearance. Undigested stools, perhaps a little better. Other symptoms no change. Continued same treatment. After this prescription, he rapidly improved, and took no more medicine. I frequently meet him on the street, and he has remained well up to this date, March 1st, 1880.

The symptoms which I deemed characteristic of Oleander were the intense itching, *worse while undressing*, which I have frequently corroborated, and the undigested stools.

P. S.—I ought to say that the potency was the 200th centesimal, made by Tafel, and by hand.

H. NOAH MARTIN, M.D.,

Philadelphia, Pa.

Mr. Geo. A. M., aged about 30 years, wholesale grocer, active habits; lost his mother a year ago, and an infant child six months ago; exerted himself much in nursing. Complains of shortness of breath, bruised soreness, and stitching pains in region of left ventricle of the heart.

Percussion, shows cardiac dulness, extended to the left nipple-line and somewhat beyond. Palpation: excessive action of heart; impulse of apex, very strong to nipple-line, and upward to above nipple. Auscultation, inconstant blowing sounds at apex, and from aortic orifice to great vessels, momentary in duration; no continuous valvular murmurs. Face pale and anxious; eyes appear somewhat fixed.

March 26th, 1880. I prescribed Arnica²⁰⁰, night and morning (No. 40) pellets, three at a dose).

March 30th. Feels much better in every way. The heart is more quiet and less sore. R̄. Sac. lac.

April 5th. Still better; cardiac dulness less extensive; impulse less extended upward, and to left. R̄. Sac. lac.

April 11th. Better. R̄. Sac. lac.

April 22d. Better as to heart, but has been moving; exerted himself going up and down stairs, and was exposed to cold. Now has pleuritic stitches in the right side of chest, worse by inspiration and motion. R̄. Bryonia²⁰⁰, three times a day, three days, followed by Sac. lac.

May 1st. Complains of nothing; feels quite well. Physical examination shows the cardiac dulness and apex impulse normal; sounds normal. Discharged.

J. C. MORGAN, M.D.,

Philadelphia, Pa.

No. 1. A boy, aged 7 years. Convulsions; starts from sleep, with red face, frightened looks, staring eyes, and convulsive jerkings of the body. *Bellad.* 6^m (made by hand on the Hahnemannian plan), relieved. A relapse the next day yielded to a dose of similarly prepared 10^m. Had had convulsions before, but has had none since (one year).

No. 2. Miss —, under treatment for ten months. Some symptoms yielded, but the following resisted the usual remedies. Ineffectual urging to urinate; when it does pass it comes in drops with much straining and intense pain; worse before menses. Headache the third day of the menses, the flow suddenly stops, then the headache ensues. Dim vision; worse when stomach is out of order.

Lith. carb. 10^m Fineke relieved promptly.

E. A. FARRINGTON, M.D.,

Philadelphia, Pa.

June, 1879. Etta S., aged 14 months, bright, healthy appearance. Chill in the morning, two hours earlier every other day. Awakens bright, becomes thirsty, after a little time falls asleep, has a marked chill, blue nails, fingers and tips, followed by high

fever, flushed face, somewhat restless, then profuse and general sweat, after which she awakens in good spirits. Opium²⁰⁰, Nux vom.¹⁰⁰⁰, Bry.²⁰⁰, etc., failed. Nux mos.²⁰⁰, a powder every six hours. No more chills to date. While sleepiness is a characteristic of Nux mos. I find no mention of the *profound sleep during all the stages* of an intermittent fever, which was the characteristic symptom of this case.

I cured a case of three years' standing in the same family with Cimex²⁰⁰. Characteristic symptoms, drawing under the knees, and sensation, during the chill, "as if the body were contracted."

J. B. GREGG CUSTIS, M.D.,

Washington, D. C.

A gentleman, aged 35, called on me to prescribe for a facial neuralgia. The indications being for Arsenic, I prescribed one dose Arsenic 200 Fincke. He clapped his hands to his head in intense agony, and cried: "Doctor, why did you give me Arsenic? I ought to have told you that I never can take Arsenic." He remained in intense agony for about five minutes, when the pain disappeared, and his neuralgia never recurred. He had no means whatever of knowing what medicine it was that I had prescribed.

A. C. COWPERTHWAIT, M.D., PH.D.,

Iowa City, Iowa.

An old man, seventy-five years old, but otherwise hale and hearty, a good eater, complained for about one month of dizziness, with tendency to fall on left side; he fell once or twice when walking in the street, but never lost consciousness or, if so, only for a second or two. All secretions and excretions normal; radial pulse, 60, hard and tense, perhaps from atheromatous condition of arteries.

Vertigo with inclination to fall to left side is found in Aur., Cil., Dros., Euphorb., Lach., Mercur., Mez., Sulph. zinc.

Remedies for aged people, Ambra, Baryta, Con., Opii, Sec.

The indications for Aurum met. were clear, and in the 200th potency, a dose morning and evening, for a few days, relieved

the vertigo entirely; the removal of the atheromatous condition was out of the question.

SAMUEL LILIENTHAL, M.D.,
New York, N. Y.

AGGRAVATION FROM RUTA GRAVEOLENS 200th.

On February 2d, 1872, I was consulted by Mr. B——, about fifty-five years old. He has for years been subject to prolapsus of the rectum. During stool, and even when *lifting* hard, the bowels would protrude to the extent of an inch or more. He was in the habit of evacuating the bowels soon before retiring, as it was difficult to retain the bowel for some hours after stool, unless he kept in a recumbent position. After a careful comparison of all his symptoms with those of *Ruta graveolens*, I gave that medicine in the following manner: Twelve powders of *Sac. lactis*, numbered. In Nos. 1, 4, 8, and 12 were a few pellets of *Ruta* 200th, the others were blank, but also contained blank pellets, that they might all appear alike. (This is my usual custom when giving blanks and medicated powders.) A powder to be taken at 10 A.M. every day. After the powders were all taken, he reported that on the days he had taken Nos. 1, 4, 8, and 12, he had several diarrhœic stools, followed by such general prostration and weakness of the bowel (as he expressed it), that he was obliged to keep in the house, and very quiet for twenty-four hours, as even slight exertion would cause a descent of the bowel.

Left him without medicine for a few days, and wrote to Dr. William Eggert, for advice, and requested him to send me *Ruta* very high. Dr. Eggert advised me to wait on the action of the 200th, as an aggravation was generally a pretty certain indication that the correct remedy had been given. He had no higher potency of *Ruta* to send me. On the 24th of same month, twelve days after the last dose of *Ruta*, no improvement having taken place and not being fully satisfied that the medicine had caused the aggravation, I gave him a similar package of powders, only numbering the medicated ones 3 and 9. Again he reports that Nos. 3 and 9 were the same remedy that I gave him before in Nos. 1, 4, 8, and 12, causing exactly the same train of symp-

toms, and begging me to give him no more of that kind, as it made him sick.

From this time on to April 5th following, I kept him on blanks, numbered as before, but no improvement following, I gave him another dose of *Ruta* 200th, with precisely the same result, and he discontinued treatment.

In this case there was no possible chance for the patient to ascertain beforehand any difference between the powders, as all appeared and tasted alike, and he did not see them put up.

The *Ruta* was Tafel's 200th, the only preparation of the remedy I then possessed.

A. L. FISHER, M.D.,
Elkhart, Ind.

TYPHLITIS.

Patient, stout healthy boy, aged twelve years, dark hair and eyes; on November 1st, 1879, swallowed a leaden bullet, larger than a No. 80 globule, and weighing 75 grains, or nearly 4 scruples.

For a week afterwards suffered no inconvenience except constipation; bowels moving but once during that time. Was called to treat him November 8th for pain and tenderness in the bowels, worse in right iliac region, loss of appetite, white furred tongue, slight nausea, feverishness, pulse 90, temperature 100, thirst, but not for large quantities of water. The pain was not spasmodic like colic, but a constant sore pain, aggravated by motion. Patient could not tell if the bullet had passed. Bowels had moved but once since it had been swallowed, and not at all for three days.

Was this a case of lead poisoning? or had the foreign substance anything to do with what appeared to be enteritis?

Nothing else could be assigned as a cause; no cold, or imprudence in eating or drinking. Gave Plumb. met. 50^m, in water, a dessertspoonful every two hours, till three were taken, then if better to be discontinued. Next day patient easier; medicine had been given irregularly during the night, as the sleep was disturbed. Pulse still 90, temperature much the same. Medicine continued, three powders, three hours apart. In twenty-four

hours the case was still further improved ; pulse and temperature lower ; soreness not so great, and more circumscribed. Three more powders of Plumb. were given, three hours between. Did not hear from patient again for two days, when a message came that he was much worse. Found him lying on right side, with right limb drawn up ; could not be moved without great pain. Complained of a hard, sore lump, the size of a man's fist, in the right iliac fossa. Pulse 100 ; temperature 100 ; disgust for food ; tongue white ; nausea ; urine very slight, and dark brown ; constipation, no action of bowels for seven days. Gave Lyc.^{em} in solution, dessertspoonful every two hours, unless better. Next day, much worse ; had had a bad night, no sleep, pulse 110, temperature 101 ; constant moaning and screaming with pain in the lump, which was now as large as a man's two fists, hard and very tender, with slight discoloration of the surface. Everything taken into the stomach was now rejected ; vomiting had continued for twenty hours. Besides the slight nourishment taken there was a green ropy fluid thrown up. Still no action of the bowels. Tepid water injectments had been given, but passed off without effect. No urine passed for twenty-four hours. Friends were anxious. Allopathic physicians, who heard of the case, predicted death certain, and not far distant. An abscess evidently seemed about to form in the region of the obstruction ; and how was this to be averted ? The bowels must move, or the patient will die, say the allopaths, and the allopathically inclined ; but how is this to be brought about ? By the hazardous operation of a cathartic ? a failure of which to effect this end would only add to the pressure and inflammation, and thereby insure certain death.

Is this a case where the law of the similars is suspended or inapplicable, and the physician justified in resorting to anything known to the medical world to save the life of the patient, an only child, and criminally culpable if he did not ? Can we conceive of a more complicated or dangerous condition, or one that it would seem was more beyond the sphere of medication ? But even in this dreadful state of affairs, homœopathy, pure, rigid, and *unadulterated*, is the only auxiliary required. Hepar sulph., 1000th (Tafel), in solution, was given, a dessertspoon-

ful every two hours for twenty hours, without intermission, by which time the vomiting had ceased, the pulse fallen to 105, temperature 100, urine passed freely, pain and expression better, lump softer and not so tender, medicine continued at longer intervals. Next day, the ninth of the treatment, very much improved; pulse 100, temperature 100, had slept three hours during the night, taken and retained nourishment; medicine continued every four hours. In twenty-four more pulse 95, temperature normal, pain and soreness greatly relieved, bowels had moved freely, filling a gallon chamber one-third full, in which there were eight or ten hard dark balls over an inch in diameter, and covered with slime; the balance varied in color and consistency, a part being brown and thin, and all very offensive. Unfortunately this stool was destroyed without being carefully examined for the bullet, which very probably had also passed off.

This was the tenth day of the treatment, and the thirteenth day from the time the bowels had last moved. The tumefaction and soreness now gradually subsided, and a permanent recovery followed.

The only reason for giving the medicine in the 1000th potency was that I did not have it any higher.

C. PEARSON, M.D.,
Washington, D. C.

APPENDIX.

DISCUSSION.

S. LILIENTHAL, M.D., New York : To set the ball in motion, as it seems to be at a standstill, and nobody wants to get up, I will say a few words. The first thing I want to say is that since Professor T. F. Allen, after sending in his report, wanted to make his own provings, he wanted to select his own medicine. It was understood that Dr. Sherman was to make them. After he was informed of this he withdrew. I must say that I have felt perfectly happy the whole day at the report which was made by the Bureau of *Materia Medica*. We have found out that chemistry fails entirely in proving the value of high dilutions. It was acknowledged that the spectroscope fails. We have found out also that the microscope fails to answer the question as to high potency. I thank my friend Dr. Sherman for his labor. I can only see from his report that there is a difference between the well body and the sick body, and that we often find a reagent in sickness to which our high dilutions are especially attractive. For instance, you might have a proving on the healthy body and find that they are acting perfectly well. On a sick person it would be different. I believe this is the fact without doubt. Professor Dake, in his first part, acknowledged that neither chemistry, nor the microscope, nor the spectroscope has ever found a difference in the blood, whether it be a miasm or anything else. There is then a higher power or force, an elevating force behind those germs, or a life force, if you please. Nobody has ever seen it. We know it only by its effects on the body. We cannot hold it, we cannot take it in our hand. That was acknowledged by Professor Dake. He acknowledged that the spirit is not recognizable. The papers of Professor Cowperthwaite and of Dr. Lawton, were most satisfactory. One of the most beautiful expressions which I heard was given by Dr. Lawton. He said : "There is the soul of the drug acting, we are all aware, in the living person." Now nobody knows where

that soul is, yet they know it is in us. It works in us. It is constantly at work. Just such a soul, if I may use the expression, is in the drug. It works; but you have to liberate it, and by being liberated it works quickly, immediately, and we see the benefit of it.

Now comes the question, why do not others see it? Macht's nach, macht aber genau nach. Try it, but do not say it don't work. You may say you do not understand it, that will be a free confession. You may say you do not know how it works, that may be the truth. You may say I do not understand a great deal of physiology, I only know the effects. Just so you see the results.

I only beg of you, those who have a prejudice against high dilutions, to be sure you are right. That is, be sure you are right in the selection of a remedy. Be sure you are right in your diagnosis of the disease, and that is the *totality of the symptoms*, and nothing else. All the pathology, live pathology, I do not mean pathological anatomy, but living pathology, is nothing less than what Hahnemann so beautifully called it, the totality of the symptoms. You get the totality of the symptoms and you cover them carefully, you cover them with the right remedy and you will always cure your case. If your case is not cured, you may be sure that you are at fault. The trouble is not in the dilution but in your judgment.

DR. ADOLPH LIPPE, Philadelphia, Pa.: Mr. President, permit me to make a few remarks on the paper read by Dr. Sherman. He seems to be in error when he reflects upon people whom he calls believers in high potencies, who have not taken any part in these proposed provings, because, impliedly, he meant to say that they were afraid that their belief would come to grief. Those that have used medicines according to the rules laid down by Samuel Hahnemann, are not believers. They are convinced of the effect of the smallest dose. It is not a belief, it is a conviction—conviction obtained by experiment. It is for that reason, and that is one of the reasons, why they have taken no notice whatsoever of that proposed reprovng of the 30th potency. But if Dr. Sherman had been good enough to read his homœopathic literature, he would have found that that experiment had been made in Vienna thirty odd years ago by Dr. Watcher, on *Natrum muriaticum*. He found that *Natrum muriaticum* cured the disease in the high potencies. What was the result of all that proving? Dr. Watcher was anxious to prove the 30th potency. He did not believe in it. The experiment was made, first on the healthy and then on the sick. He was honest enough to say, that he was grieved to be compelled

to acknowledge that the 30th potency was of great efficacy in producing conditions on the healthy organism, and in curing sick people in a very short time. More so than the lower potency. We can say that at that time it was settled.

Now here we are, thirty years later, called upon to prove that over again. Now, if Dr. Sherman was very anxious to do so, he might have followed the plan of Dr. Watcher—made his first provings of the potency upon the healthy, obtained all the symptoms thereby, and obtained a cure. But Dr. Watcher not only produced with the 30th potency many symptoms, but forty times more symptoms than he had produced with the lower potency. He corroborated Hahnemann's *Natrum muriaticum* symptoms, and added materially to the knowledge of the inert drug substance. That is the way to make provings. It is the plan proposed, but it has not been responded to, for the very reasons that I have now stated; not because there was any particular objection to it, except the objection that it was useless, and our time can be better employed by developing homœopathy than by proving and reproving again that which was known thirty years ago to be true.

ALVIN E. SMALL, M.D., Chicago, Ill.: I was not present, unfortunately, when the reports were read on *Materia Medica*, but it struck me as being rather queer, when I read the title of the papers given here, as furnishing, for instance, a test for chemistry. Now, if we want a physiological proving, what has chemistry to do with it? We do not want to prove our remedies by chemistry, we want to prove our remedies on the living subject. We trace them to vital conditions, and be sure we regard the law. We want to be sure how far the system is impressible to the action of remedies. We cannot test or measure a remedy by chemistry, neither can we by the microscope or the spectroscope; we cannot prove them by these instruments. It is only on the living subject that we can prove them. We know that these living subjects are susceptible to a variety of changes which exist in nature, which chemistry can never touch, and of which no measurement by any instrument which you can invent can be made.

I will ask you what makes the oak grow? You can plant an acorn in sand or shot and water it with rain-water, and it will extract nutriment from the rain-water. The nutriment is as infinitesimal as the highest potency you can name. The tree will be elaborated, and when sufficient rain-water has been furnished this acorn will germinate, it will grow, and produce a tree, and it will spread its branches, and if you will take the pains to analyze that tree you will find many chemical elements in the tree

which you cannot find in the rain-water. There is a singular metamorphosis which takes place in living beings. Now we have read of the lichen which feeds on the atmosphere, and yet it sends out roots, and will elaborate itself and become a vegetable and a plant that can be seen by the naked eye. Now where did it come from? where does this impress come from that is made upon the living tissues? When the plant has developed itself it yields, on analysis, the same chemical elements that you will find in other bodies of nature. Nature is full of these forces, and a remedy addressed to the vital forces will produce a modification of the system, so that no estimate can be obtained by measurement. We have seen this. I would like to relate a fact which I saw myself, in Philadelphia.

I was called to see a lady there, who said to me: "Doctor, don't you dare to give me any Pulsatilla; it always distresses me so that I do not wish you to give it to me." Some time after this I was in attendance upon her, in a fit of sickness. Her symptoms seemed to justify the prescription of Pulsatilla. I recollected what she had said, and I selected a very high potency—Jcnichen's potency—such as I thought she never would detect, even if I administered it to her. I gave it to her and said nothing, and I mentioned it to no person whatever. The next time she saw me she said:

"I told you never to give me any Pulsatilla; I never have the feelings that I experience except when I take Pulsatilla."

Now that was the 800th potency. I have seen other cases where the impressibility of the system was so great that the most infinitesimal dose can produce an effect upon the vital forces.

I once was attending a patient who was singularly affected by the odor of musk; she could never endure the odor of musk without its superinducing a fit of syncope. Now all philosophers agree that odors are small particles escaping from odoriferous bodies. You may think it strange that the odor of musk would produce this syncope upon a patient, but it is a fact. You may call it idiosyncrasy, or call it an impressible condition of the system, but it is a fact nevertheless. The patient becomes affected with the musk, and experiences a fit of sickness. How many patients have we seen affected by the odor of a rose or some other flower? We know that this odor produces an action upon the living economy, and there is no kind of measurement that can be inflicted upon the dose that has been administered.

F. R. McMANUS, M.D., Baltimore, Md.: My friend, Dr. Small, has been speaking about the impressibility of the human system. I knew a lady who lived in Baltimore, 35 years of age, and there was nothing that seemed to disturb her equanimity.

She was in ordinary health, but the odor of a shrub affected her seriously. Experiments were tried by her astute allopathic physician, to ascertain if he could not bring a shrub into the room without being detected by her, but it always produced syncope; she could not pretend to handle it at all, or to smell it. For fear he would be mistaken about it, Dr. John Taylor, who was the physician of Baltimore in the olden times, spoke to me of the case; he was her physician. He took a shrub and an ordinary-sized pocket handkerchief, and he put it in one corner and folded it, and wrapped it, and wrapped it, and wrapped it, and folded it together, and then he wrapped around it a silk pocket handkerchief, and he put it in his pocket. He was attending her professionally; he visited her. She said to him:

"Doctor, I feel very weak."

"Well," says he, "what is the matter; are you in pain?"

"No, I have no pain, but I feel weak; I feel a weakness coming over me."

"What kind of a weakness; what do you suppose produces it? I am only feeling your pulse."

"Well," she says, "I have a feeling such as I always have from the odor of shrub."

Now that is the history of one case that I heard of in my life, but it is a fact. It only proves the impressibility of the system.

THE PRESIDENT: Several gentlemen wish to know the name of the plant. What was the name of the plant?

F. R. McMANUS, M.D., Baltimore, Md.: The common garden shrub, *calycanthus*. That was an extraordinary case; I mentioned it as corroborating what Dr. Small says.

WILLIAM OWENS, M.D., Cincinnati, O.: In regard to the report which has been presented by the Bureau of *Materia Medica*, I must say for myself that I have been actually disappointed. I had expected to hear a reference to some drugs in the report, and their application on the human organization, as we understand it, in their relations to homœopathy. I expected to hear a report upon drugs, and their relation to the human organization, which is the proper function of the Bureau of *Materia Medica*, Pharmacy and Provings; but instead of that, sir, I hear a report here on physical science—physical science, and not *Materia Medica*. The first part of the report appertains entirely to physical science and the presence of medicinal substance in certain attenuations. It gives no relation of those substances to any morbid condition of the organism whatever. And so, therefore, I am somewhat disappointed in the report, although, from the caption of the several papers, I should not have been.

Again, sir, I find in the other department a report of a series

of clinical cases properly belonging, I presume, if the chairman will allow me to make the statement, properly belonging to the Bureau of Clinical Medicine. According to this, therefore, we have no Bureau of Materia Medica, Pharmacy and Provings before us for discussion.

This is pharmacy without a pretension to pharmacy. If this is pharmacy, then we have no comments perhaps to make. But this is simply a report upon physical science; the report relates simply to physical science and nothing else. I had expected, Mr. President, from the character of the gentlemen engaged in making this report, that we would have had something else, or something said in relation to drug action, and to the action of drugs on the organism, what tissues are affected by drugs, how they are affected by drugs, what are the results of these effects, and from that, an inference as to the power of drugs in disturbing the functions of the organism. But not one word has been said upon that subject. If I understand the papers correctly, I believe not one word has been said upon that subject.

The chairman of that bureau did, in a paper which he read, refer to this matter, but they did not appertain to the papers proper of the bureau. With all credit to him, he has relieved the bureau from an onus which I think could properly attach to it under the circumstances, that is, of not making a proper report of Materia Medica. First, we should know where the drug impinges, what tissues are disturbed, whether the irritation occurs within the blood, whether it occurs within the nervous system, and if in the nervous system, which class of nerves it disturbs. Is it the cerebro-spinal, or the sympathetic, or the organic, or the vegetative? These are the points really which this Bureau of Materia Medica ought to make. When we can establish a basis for their action, and a uniform prescription, a uniform dose may be more nearly approximated. But until we can lay the foundation for uniform drug action by attaching that action to certain tissues of the body, we utterly fail to perform the duties which we owe to the homœopathic medical profession. Until, I say, we can accomplish this, we utterly fail in the duty we owe, as teachers and students of Materia Medica, to the homœopathic profession. I believe, sir, that we will have to go back and re-study many of our physiological problems, and some of our histological problems, and when this is done we shall be able to draw conclusions from the facts there stated quite diverse from those offered by our old-school physiologists and pathologists; draw conclusions which will lead us in quite an opposite direction, and afford a beacon-light which will lead us onward to the study of true homœopathic Materia Medica. I am sorry that I cannot say that this bureau has furnished us a stepping-stone to this

field, or taken the first step in that direction. It has only proved that certain medicinal substances are inert when given in the 1,000,000th potency, which was known thirty years ago.

J. H. McCLELLAND, M.D., Pittsburgh, Pa.: I cannot say that I am discontented with the report of this bureau. In the first place, I read on the circular, that it was to be a report of the Bureau of *Materia Medica*, Pharmacy, and Provings. In the second place, I read that the bureau, as was their right, selected a certain subject, and that subject related to the proof of drug presence and power in attenuations above the 6th decimal: first, as furnished by the tests of chemistry; second, as furnished by the spectroscope and microscope; third, as furnished by the tests of physiology; fourth, as furnished by analogy from the field of impalpable morbid agencies.

Secondly, the proofs of medicinal presence and efficacy in attenuations above the 6th decimal: first, as furnished by the tests of clinical experience, in the use of attenuations, ranging from the 6th to the 15th decimal; second, as furnished by clinical experience, in the use of attenuations, ranging from the 15th to the 30th decimal; third, as furnished by clinical experience, in the use of attenuations, above the 30th decimal.

Now, you might as well expect the Bureau of Surgery, after it had selected the subject of tumors, to bring in a report on fractures, just as well. I think no one will doubt that this bureau has adhered strictly and logically to the subjects selected by the bureau, and which it had a perfect right to select. Next year they may take up another phase of *Materia Medica*. This year the subject was certainly very ably handled, and in reference to that report I would like to say one word. We have a good deal of testimony, as I remarked yesterday, in regard to another paper, of a negative character. We have testimony here that finds no drug in a certain potency, but that is evidently negative testimony. It proves that no drug was observed in that potency, perhaps, with the means at hand very ably handled. It does not prove that these potencies are without power, as one of the papers would lead us to believe. The positive testimony, which each and every one has had, that the potencies above a certain grade, the 30th or higher, have produced effects positively, is worth all of the negative testimony that could be adduced. But I can very readily see myself why it is that the testimony that has been offered in regard to the wonderful efficacy of the higher potencies, is received with doubt. It is because, in many instances, the enthusiasm of the reporter has carried him away really, or at any rate, has blurred his judgment. We have testimony that is absolutely defective, we have testimony as to the cure of pa-

thological conditions, that any one knows must be absolutely without foundation—without truth.

When I find, for instance, a case of typhoid fever reported, and a most frightful state of things existing, the patient just at the last gasp—a case of genuine typhoid fever—just as you all know, with certain pathological conditions produced—and a certain potency is given, and very often a very high potency, and lo! behold! the next time he is visited, in twenty-four hours, the patient is well. Now we all know, every one knows, that that cannot be. It *cannot* be. You appeal to people that ought to be reasoning people to believe things that cannot be. So we find tumors described, malignant tumors, and a high potency is given, or a low potency, as the case may be, but I really think the high potency is oftener mentioned—a high potency was given, and under the influence of the drug administered, a cure is reported. We know that a morbid growth is a thing of slow growth. It is something which is developed after months, and months, and years. When we hear the reports, we find that one dose is given, and LO! *the next day the tumor is gone!*

Now, that, I say, shocks the faith of the people in the testimony that is brought to bear as to the efficacy of high potencies. On the other hand we all know that we have given the 30th potency and the 200th potency and higher potencies, for conditions of disease which we cannot solve, we cannot limit; the conditions were progressing under their action, in a reasonable time have ceased, and the case has recovered, evidently as an effect of the medicine. Now I say, there is nothing in that testimony to convince us that there is potency and power in these 30th potencies, and higher potencies, to do such things.

DR. P. P. WELLS, Brooklyn, N. Y.: Ladies and gentlemen, I have but one word to say in reply to the statement which has just been made by my friend, Dr. McClelland. I am about to state what happened under my own eyes, and I know it is not what another has told me. It is not what another has done. I will give you the facts, and you can make up your judgment as to them. The patient was a little girl, nine years old. She was in a state of constant delirium, with intense headache. If she was asked a question she would answer it immediately, and then fall off into delirium. When questioned she declared that the pain in her head was intense. She had a severe cough with an excruciating pain through the whole chest. She had tympanites and tenderness over the whole abdomen, with diarrhoea of a brown watery substance, which was running her into the grave. Now you may call it typhoid fever or whatever you like. But

that little girl got a half dozen pellets dissolved in a teacupful of water. I observed that myself, and I know. This dose was given at 4 o'clock in the afternoon, and at 9 o'clock the next day she was sitting on the sofa dressed.

SEVERAL MEMBERS: What remedy was given?

P. P. WELLS, M.D.: I will tell you if you want to know. But that was a perfect picture of typhoid fever. That was it. The patient recovered. She was up and dressed, and sitting on the sofa. The dose was administered between 3 and 4 o'clock in the afternoon, and at 9 o'clock the next morning she was up and dressed. Dr. McClelland is mistaken as to its being possible or impossible. That which has been is certainly possible. I think we must stick by that. Now as to the susceptibility which is sometimes met with to medicinal action, a great many of us have got a great deal to learn. You have been told about the effect of a shrub. There are other medicines and vegetables which produce the same effect. If a vial of a certain medicine were uncorked and held to the noses of certain parties they would be thrown into convulsions in a minute. I have heard of an experiment that was made upon a young man which showed his extreme sensibility to drug action. Upon inhaling the medicinal substance he was thrown into convulsions. This only shows the extreme sensibility of this young man. This is found to be the case with certain persons. They are extremely sensitive.

But now I will go back to what I know myself. A patient was sent to me from a physician in New York, with a statement that she was extremely sensitive to medicinal action. The action was something singular in her case. I put a vial under her nose with pellets in it, and she went into convulsions in a moment. Now I am perfectly competent as a witness to this fact: first, that it was a vial; second, that it had pellets in it; and third, that she went into convulsions; fourth, I know that the cork was in the vial; fifth, I know that it was put under her nose; and sixth, I know that she went into convulsions just as quick as the vial came under her nose. I will speak of another thing in this patient. I would only say that things have happened to some persons that have not happened with other persons in this world, but that does not prove that it did not happen. Her susceptibility was so great, she would take a pellet in that way and be affected. She would say, "Now, doctor, I feel it going up my left arm." I gave her the medicine, and I know what it was. When it was administered, she would say, "Now, doctor, I feel it going up my arm," and she would put up both arms over the phrenological organ of hope, and say, "I feel as though all my

hope was gone out of me." I gave Pulsatilla, and her symptoms are almost a literal translation of Pulsatilla as they stand in the *Materia Medica*.

HENRY E. SPALDING, M.D., Hingham, Mass.: I would like to ask the duration of the sickness before the single dose was given—I refer to the case of typhoid fever—and what remedies had been given before that, in the treatment of the case.

THE PRESIDENT: The time of Dr. Wells has been exhausted. Dr. Brown, of Binghamton, has the floor.

TITUS L. BROWN, M.D., Binghamton, N. Y.: Mr. President, I have listened to the papers this evening, of this bureau, with a great deal of pleasure, and I am satisfied, as far as I am concerned as a member of the Institute, that they were well-presented, well-written, and well-delivered papers in fact, and a credit to the Bureau of *Materia Medica*, and the cause of homœopathy. They simply gave individual opinions according to their observations. Be they correct or not they gave them as correctly as they could. Low or high, their opinion was given to us as well as any one could give it. I think they are entitled to great credit for presenting their views fairly to us. I do not make any criticism upon the bureau or upon the papers. I rise simply to throw in a thought in reference to the action of remedies upon human bodies. You will all acknowledge that they produce poisonous effects, and produce subjective or objective symptoms, and then they produce changes of the tissues of the body. You will find afterwards just exactly how they do this. This must be ascertained, of course, by discovery.

I cannot see any difference between their physical and dynamical action. They are forms of matter, we are forms of matter, and if we take into consideration their condition and the results that they give, we can tell uniformly what the result will be. I have never found that I could substitute one remedy for another. I have found that I could cure the patient of a certain condition after having obtained the exact remedy. When I had a sufficient number of cases I had something to found the remedy on. In the same case you can repeat the same remedy. We can repeat the form, not always absolutely. You will only get a similar form—similar motion. This can be done by the action of remedies. I never produce the same effect, only a similar effect. Now what is this dynamic action? Is it not physical action but beyond our consciousness, whether it be a subjective symptom or objective symptom? We report results with our symptoms on one side or the other, which are often very hard to make out, because we do not understand them, and then we fall back and give credit to all degrees

whether of low or high dilution. The effect of the dilution is simply the action of remedies. You may give them any name you please. It is matter in contact with matter. By no possibility can you destroy any portion of the remedy. Now, for instance, I hold here in my hand a tumbler of water, pure water; from above comes a stream of pure water running into it, or you may suppose it is running into as large a vessel as you please. I put medicine in that water. How long has that water got to flow until the medicine is exhausted; until it has reached a point when it will give no dynamical action? So with the action of remedies; we may criticise, but our object is to learn. That is the object I had in coming here.

O. P. BAER, M.D., Richmond, Ind.: Mr. President and gentlemen, I am very glad to say that I recognize the fact that there are four kingdoms in the physical universe, an elementary, a mineral, a vegetable, and an animal. Most scientists claim but three, but our elementary is as large a kingdom as the rest. Homœopathy claims a portion of all these kingdoms, hence it is a physical science. Of all physical sciences it will in time be the most exact.

Now to prove some of its peculiarities. Do you not all recognize that everything in the physical universe has a form peculiar to itself, or it has some peculiar characteristics peculiar to itself? But has not everything an identity? Most assuredly everything has an identity. How do you know one from another except by its peculiarity, by its identity, by the law of comparison, by its peculiarities. It being the case that there is that identity, is it not always manifest by simple presence, or an external physical presence, a proximate presence? Sometimes this presence, whatever it is, does not come to your view; it is neither tangible nor observable. Everything has a sphere of action. Do not minerals have a sphere passing off from themselves? Do not the elements have a sphere? Do not the vegetables, and does not the animal kingdom? Everything, if you please, has something peculiar to itself, flowing out from itself; everything is influenced by that flowing out, everything surrounding it. It is its own self. It is like a grain of musk sending out its effluvia in every possible direction. So it is with every element of the vegetable, mineral and animal kingdom; they all have their characteristics. These characteristics are emanations of its life. Now this identity, I hold, you cannot destroy with all the preparations possible. There is an inherent principle within the form of the thing, seen or not; it is itself, its life. That gives it form. Life is force. Force pushes as life pushes forward and gives form to matter. The life within it is the power that that life contains. This in-

trinsie value belongs to it; it is that power which no dynamization can destroy. It is there; it is an inherent principle, inherent with the body itself that gives it identity, having, like it, a power. It may be like the musk in the tower of Pisa, that has existed for eighteen hundred years; there it is. Its presence can be perceived; it is not destroyed. Its atoms may be separated, but they are never entirely destroyed. What are the atoms? They are the most obtrusive and the most intrusive materials in the physical universe. Each atom seeks its affiliate, it seeks union. Very often we have a great combination of them, forming a molecule. This molecule is formed into masses; then they become inert; it is a finite, which is so active; it is a finite, which is so obtrusive, and as you reach the finite you reach power. It is that power which gives homœopathic medicines their virtue.

Now I don't care how much you talk about low or high dilution, the thing we want is the picture; the thing we want is that which directly corresponds to the picture presented by your patient. You have a certain class of symptoms, and you search your *Materia Medica* for a remedy that corresponds precisely, or, in other words, is a prototype which the symptoms present. There you have a *fac simile*, one with the other; one is the prototype, and the other is the type,—one is the picture, and the other is the image of that picture. Make your prescription and you succeed.

There are a few other things that I wish to speak about. One is an interesting case that I had. I was called to see an exceedingly singular case; it was a banker's wife. I was called between two and three o'clock in the morning. When I went in I found her in a perfect state of frenzy and excitement. I said to her:

"What symptom is the most prominent, can you tell me, can you pick it out?"

She told me: "Oh, I have such a very sore pain in my urinary organs; I want to urinate every minute, and I can't."

I at once took out my case of 200ths and gave her five or six pellets of the 200th of Jenichen's dilution of *Cantharis*. I gave her this to take; she took one teaspoonful.

She said: "It will not do me any good."

In less than five minutes she fell asleep. She slept for fifteen minutes, when she raised up, and I gave her a similar dose again. She told me she had no more pain. I visited her the next morning at ten o'clock, and she was about her usual work.

DR. PEARSON, Washington, D. C.: Mr. President, ladies, and gentlemen, I will not detain the Institute five minutes. I do not intend to enter into the discussion of these papers at all, but

I am thankful for small favors. I really feel like congratulating the worthy chairman of the bureau, and the members of the bureau generally, with what I regard as being very important, if not the most important papers ever offered to the Institute. Last year the papers offered on that occasion were good. The papers did remind me a little of the story told by Mr. Lincoln. On one occasion he was obliged to listen to a long essay on some moral subject with which the writer and reader were very familiar; and when he got through, and asked Lincoln's opinion of it, Mr. Lincoln said he thought that anybody that liked that kind of thing would be apt to like that kind of thing, and he would like that paper.

Mr. President, I could not help thinking of that anecdote, and I compared the members of this bureau to the unruly church member, who was eternally backsliding and as often being brought into the church and baptized over again, until a sailor suggested that the only way was to put him in and keep him in over night.

You will remember, Mr. President and gentlemen, that on that occasion I predicted that there was to be a reformation of this kind. I must confess I was sold on that occasion, but I felt just as good as a Methodist preacher in camp-meeting.

T. C. DUNCAN, M.D., of Chicago: I move that this bureau be closed.

THE PRESIDENT: We will hear from Dr. Dake first, as he is the chairman of the bureau.

DR. J. P. DAKE, Nashville, Tenn.: Mr. President and gentlemen, I will endeavor to close it up very rapidly. In the first place I wish to correct my friend Dr. Lilienthal with regard to Dr. Allen's experiments. The bureau never represented to Dr. Allen that Dr. Sherman should make the attenuations for his test; they never proposed that to him at all. The proposition was that these preparations should be made in New York city by a committee, as I stated in my introductory.

My friend Dr. Small was not present when our papers were read; if he had been he would not have made the speech which he made here a few moments ago. He says he don't want chemical provings. Now those gentlemen who heard our papers know very well that we did not insist upon or limit the proving of medicines to chemical tests, by any manner of means. Indeed, we went far beyond that, even into physiology, and even into the sick-room.

Now I am perfectly satisfied with what this bureau has been trying to do, and I will briefly recapitulate its work.

This bureau presented, last year, first a history of Hahne-

mann's object and methods in drug attenuation. I challenge any gentleman to successfully contradict the statements made in the first paper giving this history. It was correct.

In the second paper we gave the history of drug attenuation as practiced by the followers of Hahnemann since his time, with a special reference to deviations from his method. I say that the statements made in that second paper are correct, and when they are objected to I will be obliged to any gentleman if he will point out to the author of that paper the misstatements of history.

The third paper presented was upon the means and methods of drug attenuation with reference to impurities and errors; and this paper was very carefully prepared, stating the means used and the methods pursued, with the dangers of impurity and inexactitude and evil in our pharmacy. It was a valuable paper, as you will believe when you read it; when you see it in print you will appreciate it. That was as far as we were able to go last year on account of the want of time. We then deferred, as I mentioned in my introductory, the remaining sections of our subject to this year. We have presented papers on them to-day. We have adhered to our subject, we have stuck to our text, and I believe you will agree with me that we have not wandered away.

We have to-day presented the evidence of the presence of drug matter and drug power in homœopathic attenuations. Have we endeavored to destroy or to tear down? Not at all. We have endeavored from all quarters to gather, by all tests, proofs of drug presence and drug power in drug attenuations, not only in the 1st, 2d, 3d, and 6th, but also in the 12th, 30th and 200th, and as much further as any test can possibly go, be it in the physiological laboratory, or be it in the sick-room.

Now we have endeavored most faithfully to adhere to our plan; we have done it with the best of feeling in the world, wishing to upset no man's faith. We have not worked for the profession alone; we have worked for homœopathy and for the medical world and the world outside. If we wish to know anything in regard to drug attenuation we have to study and experiment to find it out. That is what we have been endeavoring to do, gentlemen, and we have done it, as I have said, with the best of feeling. It has been our aim to develop the truth, faithfully and fully, not to the injury of one party, nor for the benefit of another. In the bureau we have not all been of the same mind on the question of drug attenuation; but we have all endeavored to be honest and fair. I will not repeat. (Tremendous applause.)

T. C. DUNCAN, M.D., of Chicago: I renew my motion to close the bureau.

DR. HAWKES, of Chicago: I rise to a question of privilege. My name was mentioned in one of the papers, and it has left a misapprehension on the minds of my friends in the Institute. This would be the case if that matter was not cleared up. The intimation was made, or impression given, to some of my friends that I had been afraid to make a test. My reasons were these, and I think they are good ones: that in a matter of such national, such world-wide importance as the settling of a question of potency, that it was too great to be placed in the hands of less than representative men. In proof of the correctness of my assertion, the very first report of the Milwaukee test committee contained two gross errors in connection with my name. One was a positive misstatement, and the other was an implied misstatement. These facts I pointed out in a subsequent statement. I think that this also will give the reason why Professor Allen dropped the matter where he did. My friend, Professor Allen, I think will give the same reason.

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